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Essays and Cases.

Art. I.—Irritation and Inflammation of the Peritoneum.

Cases of Irritation and Inflammation of the Peritoneum, from the impress of Foreign Matter; with general remarks on the Pathology and Treatment of Acute Intestinal Diseases. By Daniel Drake, M. D.

Read before the Cincinnati Medical Society.

The painful affections of the Alimentary Canal, below the diaphragm, which, left to themselves, or resisting medical treatment, prove speedily fatal, are seated in one or more of three different tissues—the mucous, the muscular, and the serous.

In a majority of cases, they begin in one and extend to the others. Thus, when a large dose of tartar emetic or corrosive sublimate is received into the stomach, an irritation is instantly established in the mucous membrane, which throws the muscular into unwonted and irregular contractions, expelling the contents of the tube, both upwards and downwards, and attended with excruciating pain. The irritation is speedily transmuted into inflammation, but this is followed by no abatement of the spasms of the middle or muscular tunic, which, in most cases, are sustained equally by nervous, and

nervo-vascular or inflammatory irritation. Again, when the serous membrane becomes the seat of irritation, accompanied or not by inflammation, and the offspring of causes acting either directly on it, or indirectly through some distant part of the body, the muscular tunic is immediately affected. Its regular peristaltic action is, in general, replaced by painful spasms, which sometimes eject the contents of the stomach and upper bowels, but scarcely ever carry downwards the matters contained in the latter. The muscular tunic may, however, be affected before either of the others, although beyond the reach of foreign matters. This results from its sympathy with other parts of the body. Thus, a disgusting smell or taste excites vomiting—fear increases the peristaltic action—a tobacco poultice to the abdomen does both—and cold to the surface of the body originates in it the violent and irregular contractions, which constitute one of the varieties of flatulent colic.

When the peristaltic function is deranged, it occasionally happens, that one portion of the intestine is drawn into another, either upwards or downwards; should the receiving part not compress it, this accident will do no harm; too often, however, a spasmodic action establishes such compression, when the pain is immediately augmented, and an inflammation begins, which may affect one or all the tissues.

From the rank and value of the alimentary canal in the economy of every animal, we might decide, *a priori*, that the morbid states to which I have referred, must speedily involve the whole organism; and such we know, from observation, to be the fact. In the first stages of all these affections, whether commencing in the mucous, the serous, or the muscular tunic, we invariably find the powers of the heart and brain depressed, and exercised in an irregular manner; and sometimes this depression is so great, as to destroy life in a single day or less. In these cases, but little inflammation is developed, and the principal morbid appearances found on dissection, are those which connect themselves with the distribution of the flatus of the bowels. Oftener, however, the patient lives long enough for nervous irritation to pass into nervo-vas-

cular or inflammation, and its consequences are revealed by dissection after death—in most instances by the symptoms during life. In many cases, however, the action of the heart is frequent and *feeble*, and the developement of heat inconsiderable throughout the whole course of the inflammation; effects which arise, perhaps, from the nausea so generally present, and which, when induced by the mildest artificial means, never fails to lower the powers of the heart and brain, and reduce the energy of the calorific function. Hence results the mixed and uncertain diagnosis of these affections. Nothing, indeed, is more difficult, than to decide accurately on the real condition of the diseased parts, till examinations are made after death. Let us enumerate some of the symptoms:

1. *Pain.*—This continues from the beginning till near the termination of the disease, whether inflammation supervenes, or the patient dies from depression merely. The ordinary method, external pressure, employed to determine whether inflammation be present, is no doubt entitled to consideration; but I cannot regard it as infallible, having seen cases where it afforded relief, while the symptoms, the appearance of the drawn blood, and the beneficial effects that followed its abstraction, evidently indicated inflammation; and on the other hand, having found it insupportable, when, from the termination of the case under a course of treatment not adapted to the cure of inflammation, that condition could not have been developed to any decided extent. Still, the general rule is no doubt founded on fact; and the exceptions may hereafter, perhaps, be explained.

It has been said, that as long as inflammation is not present, the patient is restless and tosses himself in bed; but when the abdominal tumefaction becomes great, he will generally maintain one position, whether inflammation exist or not. It has also been thought, that a criterion is furnished by the paroxysmal character of the pain, it being decidedly such when inflammation is not present, and more continuous when it is. But this is a conclusion on which little reliance should be placed, for the pain is generally paroxysmal, as we find it in puerperal peritonitis or hysteritis, and in both cases depends

on the same cause—muscular contraction. Whether the patient dies with, or without, inflammation, the pain continues till near the close of life, and ceases only with his expiring sensibilities.

2. *Nausea and Vomiting.*—Both of these symptoms, or the first only, is generally present, and cannot be considered as pathognomonic either of mere irritation or of inflammation.

3. *Constipation.*—This commonly exists throughout, and has no necessary connexion with any thing but an altered muscular function, which we have seen exists as an essential circumstance in every case.

4. *Abdominal Swelling.*—This is perhaps always present, and cannot be said to afford any special indication.

5. *Rigidity and hardness of the Abdominal Parieties.*—This is probably the same in both cases, unless the inflammation extend to the *peritoneum abdominis*, and affect the muscles, when it is always greatest.

6. *Thirst.*—This, likewise, is generally, if not always present; but is more urgent when inflammation has set in.

7. *Initial Chilliness.*—This is perhaps greater in cases which assume an inflammatory character, than the others; but decided rigors are often present in mere nervous irritation.

8. *Febrile Heat.*—When the patient is of a sanguineous temperament and a vigorous constitution, the heat is well sustained, and often excessive after the developement of inflammation; but in opposite temperaments, and feeble constitutions, a natural temperature may consist with decided inflammation.

9. *The Pulse.*—I have already spoken of this. In all cases it is generally small and frequent; but I have seen it preternaturally slow, when there was, and when there was not inflammation; and have more than once known it extremely feeble, while destructive inflammation was rapidly advancing.

10. *Hysterical Symptoms.*—It might be thought, that the presence of these would indicate a state of pure nervous irritation; but they often show themselves when inflammation actually exists, though the more intense that condition, the less violent are those symptoms; because intensity of inflam-

mation indicates a temperament not favourable to hysteria.

11. *The Mode of Fatal Termination.*—The final cessation of the symptoms in death, does not instruct us fully on this point. The closing scene is indeed nearly the same in all cases. Excessive tumefaction of the abdomen, preventing the descent of the diaphragm and the action of the abdominal muscles, consequently, producing great difficulty of respiration, and confining the patient to his back, with his head and shoulders raised; cessation of pain; sinking of the pulse; reduction of heat; and, in general, some degree of delirium, equally presage the fate of the patient, whether he dies without the supervention of inflammation, or, that condition having occurred, it passes on to gangrene or not.

This uniformity of the closing symptoms, when the *post mortem* examinations which follow, disclose in different cases—first, no inflammatory lesion—second, inflammation producing effusions of lymph, serum, or pus—third, the same state, passing into gangrene, deserves to be considered. Let us begin with the last condition. The greater number of those who die from abdominal inflammation, do not exhibit any gangrenous appearances on dissection. Such inflammations, then, may terminate fatally, without running into that state. But the deposites and agglutinations found among the bowels, would not, as observations made on chronic peritonitis demonstrate, prove fatal; it is not then to them, any more than to the gangrene, that we should ascribe the death of the patient. From what then does it result? Manifestly from the lesion of the functions of the entire organism, by the intensity of the morbid condition of the abdominal. They suffer depression and overthrow, the vital forces become exhausted, and the phenomena of life can no longer be emited. The symptoms of the closing scene are not, then, the *consequences* of effusion, or of gangrene, states which may or may not be present; but of the pathological condition common to all fatal cases, whether inflammation happens to supervene or not. Hence we come to understand, how three patients, who might, after death from abdominal disease, exhibit respectively the effects of irritation, effusion, and gangrene, may, in the

close of life, present entire uniformity of symptoms—abdominal inflation, abatement of pain, obtuseness of sensibility, reduction of heat, and vacillation of pulse. These are, in fact, the signs of a fatal exhaustion of the vital powers; and whether, before this stage arrives, the morbid action had led to extravasation, or to the death of a portion of any of the tissues, we have no means of judging till after death.

Let us proceed to discuss some of the general principles which should govern us in the treatment of these affections.

1. When the remote cause is a material and palpable irritant from without, introduced upon the mucous or the serous surface, its removal is a *sine qua non*. Unfortunately this is seldom or never practicable, in reference to those which act upon the peritoneum, and can be accomplished but imperfectly, when, having passed the pylorus, they exert themselves upon the intestinal mucous membrane; for although many irritants excite in the muscular tunic an action, that carries them downwards, and leads to their expulsion; there are others which throw the peristaltic function into an irregular and disorderly action, which tends to confine them in the bowels, where, continuing to irritate, they keep up and augment indefinitely the disease of the parieties. We must not confound with these irritants, certain ingesta taken as food, which appear to be the causes of disease, or the ordinary fœcal contents of the lower bowels, seeing that these are, in fact, rendered offensive to the organ in consequence of an altered condition of its vital properties from other causes, without which they would produce none but their ordinary effects. When in perfect health, a man may indulge himself in articles of diet and drink to a liberal extent, both in quality and quantity, and experience no inconvenience, often rather the reverse; for the best health is by no means the offspring of the greatest abstinence, or the discreetest circumspection; and much experience demonstrates, that mere retention of the contents of the lower bowels, such as occurs from sedentary habits, grief, occupation of mind, or neglect, is not often, or necessarily, a cause of any of the acute diseases we are considering. From causes of this kind constipation often arises,

and continues for several days, without any other inconvenience to the patient, than pain or fulness of the head, or acidity of the stomach. It is the *cause* of the costiveness, then, that should fix our attention, not the costiveness itself; and upon this our remedial efforts should be directed.

2. We come, in the second place, to inquire into the means of restoring a healthy peristaltic function, which, if not the cure of the disease, is a sign of its cessation; but, in general, the means which regulate or restore that function are, in fact, the true remedies. Contraction is the office of the muscular membrane of the alimentary canal. When this contraction is irregular, the transmitting function of the tube is inverted or suspended, and while some parts of the canal are morbidly constricted, others are excessively dilated. On the whole there will, in general, be little or no evacuation, except by the mouth. This morbid condition may sometimes be primary, as it respects the abdominal parieties, as when the individual has an attack of flatulent colic from exposure to cold, or experiences a metastasis of rheumatism from the joints. But this spasmodic and irregular action is, in general, the consequence of a disordered condition of one or both of the membranes—mucous and serous, between which the muscular is enclosed. This disorder may be that, which, in the common parlance of the profession, is denominated a nervous irritation, or it may be an inflammation, or the former in one part of the canal, and the latter in another. Whichever it may be, as I have intimated in the first part of this paper, the phenomena presented by the muscular tunic will be nearly the same. The chief difference, perhaps, will be that in the former case the irregular contractions are more wandering, in the latter more fixed.

But let us come to the *methodus medendi*. This, with the people, is quite plain, and equally obvious to many of the profession. As the function of fœcal evacuation is suspended, and as cathartics will quicken that function in health, nothing is more *natural*, and few things more *unscientifick*, than the immediate administration of purgatives. In some cases of the mildest kind, they may, it is true, bring the peristaltic

function under their government, which implies the suspension of the morbid, spasmodic action; but in all the more violent and obstinate cases, so far from accomplishing this object, they exert on the canal no specific action, because its sensibilities are changed, and, acting as irritants, they augment the spasmodic action; by increasing the nervous irritation, or the inflammation, according as one or the other may happen, to be the existing morbid action. Let us proceed, then, to consider th emeans by which these disordered conditions of the nervous and the nervo-vascular extremities may be quieted.

3. Of nervous irritation. The means of allaying this, are chiefly those which belong to the class of narcotics—of which opium and tobacco may be cited as the most powerful. Without going into an elaborate inquiry as to the *modus agendi* of narcotics, we may say, as practical men, that they diminish morbid sensibility, and quiet the spasmodic action which springs from it, precisely the effects that we desire; for upon subduing the spasms, the powers of the system soon re-establish a healthy peristaltic function; or if they should not, we may excite it by cathartics; which, the morbid sensibilities of the mucous membrane being appeased, will prove inoffensive and speedily raise in the muscular tunic, their specific excitement. But to produce these desirable effects, they must be exhibited at once in decisive doses; and those, which, like opium and tobacco, have the least of a stimulating, and most of a relaxing and stupefying effect, will always prove most beneficial. I am aware that in cases of this kind, some writers, particularly Dr. Abercrombie, are of opinion, that certain parts of the bowel are paralysed, and therefore distended to a painful and fatal degree, and hence that opium is contra-indicated. But the same gentleman recommends tobacco, which is still more relaxing, and consequently would be more prejudicial. Moreover, whence comes the gas which produces this distension? I cannot doubt that it is the offspring of a morbid action, distinct from that in the muscular tunic; and believe that its generation may be successfully met, in most cases, by the administration of narcotics. As to the contracted parts of the intestine being in a healthy condition, and the

expanded in a morbid state, I am disposed to believe the opinion to be the reverse of the fact; for great muscular contraction may suddenly take place in the hollow viscera, as we often see in the urethra, and as suddenly cease; but paralysis, except it depend on disease of the brain, is not often suddenly induced, and never removed with the speed that we observe distended portions of bowel to recover in certain forms of colic. The recovery from paralysis is, indeed, always slow. On the whole, I think it probable, that there is temporary defective contraction of one part of the bowels, by which it fails to protect itself against distension, and excessive contraction in another part, throwing the flatus into that which makes least resistance; and that these opposite conditions may change their places, and thus lead to the transfer of the flatulent accumulations from one part to another, giving rise to borborigmi. Now, the defective and the excessive contraction of which I speak, are not distinct and opposing maladies, to be met by opposite means, but resemble the spasmodic diseases of the muscles of locomotion, from irritations of the nervous centre, in which we always observe some muscles to be in a state of relaxation or quiescence, while others are in spasm. How, then, is the case to be met? Evidently with the means of equalizing that mysterious influence, on which the muscular contractions of the bowels in health, may depend, and these are chiefly the narcotics administered in stupefying doses. These medicines, however, will not always succeed in allaying the nervous and muscular irritability, even when inflammation is not present. The system is sometimes insensible to their impress, and little or no effect will come from them. This is apt to be the case in phlegmatic temperaments; and the same condition is often connected with plethora. Whenever this state of things manifests itself, the grand resource of the profession is blood-letting. The *modus operandi* of blood-letting, in this exigency, is by increasing the sensibility and irritability of the system. Nothing does this so effectually as the loss of blood; and nothing, in general, is wanting, but this renovated vital susceptibility, to give immediate and beneficial influence to the narcotic. The value of

blood-letting, in these cases, is not always appreciated by the profession; and some of its members even dread the employment of it. But whenever nervous irritation and spasmodic action of the abdominal viscera, suddenly induced, resist the exhibition of narcotic medicines, I believe blood-letting not only safe, whatever may be the temperament of the patient, but imperatively required. To be salutary, however, it should be preceded and followed by liberal doses of opium, or some of its preparations.

4. Of inflammation. All inflammations are preceded by nervous irritation, with depression of vital power, in the parts where they arise. In some cases of intestinal disease the reduction and perversion of the vital power, are so great, as we have already seen, that they destroy life, before a sufficient time has elapsed for inflammation to be developed. These cases, however, are comparatively few in number; and whenever the attack extends beyond a single day, inflammation should be apprehended. The nature of the remote cause, the temperament of the patient, and the previous condition of his system, will determine, in a great degree, the time and intensity of the inflammatory symptoms. I have no doubt, that inflammation may be developed in some parts, while others remain in a state of simple nervous irritation. Of the difficulty of deciding on the actual condition of the parts, in many of these cases, I have already spoken. Most of those which prove fatal, are attended with more or less inflammation, and from the transient character of violent nervous irritations, I have no doubt that more or less inflammation is always developed in protracted cases, although the vestiges of it may not always be found in the mucous and serous membranes; for, as Dr. Abercrombie has admitted, it may attack the intervening muscular tunic. That gentleman has reported several deaths from colic or ileus, in which he informs us, no traces of inflammation were observed; but the general experience of the profession undoubtedly is, that in most of these cases, inflammation sets in before the patient dies. The following are the words of the writer, just quoted, in reference to this matter:—

"The usual progress of the disease in fatal cases, is into inflammation and its consequences; and we have seen it fatal while the inflammation was in various stages of its progress, from a recent tinge of redness, to extensive gangrene." *Page* 160.

In reference to the *post mortem* appearances in colic, Dr. Thomas expresses himself in these words—

"When the disease proves mortal, the usual appearances to be observed on dissection, are inflammation on the surface of the intestines, distension and irregular contraction of some parts of the tube." *Page* 368.

Of the morbid appearances after death, from the variety of colic named ileus or volvolus, Hoffman expresses himself in these terms:—

"The ileum is found convoluted, in some parts inflamed and spacelated, and the part above the inflammation remarkably distended with flatulencies." *Vol.* I. *p.* 500.

I shall not extend these authorities. My chief object is to show, that those forms of intestinal disease, which begin as nervous irritations and spasms merely, are liable in their progress to become inflammatory, and before they prove fatal, are generally attended with that condition. These make but a part of the intestinal affections which we are reviewing; but, with respect to the remainder, there can be little doubt of the existence of inflammation from an early period.

Now, this inflammation, whether mild or intense, circumscribed or diffused, scarcely ever fails to derange the peristaltic function, producing, or maintaining constipation, and rendering the bowels wholly insensible to the action of cathartics. Being a proximate cause, then, of the irregularities in the peristaltic function, it must be removed or abated, before that function can be restored. But is this important rule of practice always observed? I fear it is not. I have violated it myself, and seen it still oftener violated by others. I am persuaded, from much experience, that it is far safer to bleed, when bleeding is unnecessary, than to omit it when required. To expect to overcome the constipation, and relieve the patient from pain and distention, when the bowel is inflamed

even in a slight degree, is a crude, painful and unscientific proceeding; which, moreover, has cost many patients their lives. The substitution of active purges for blood-letting, in these cases, is, therefore, not merely a negative, but a positive injury, for while the latter would do good, the former are actually of an irritating nature and prejudicial.

Venesection is, generally, the proper kind of blood-letting for such patients; but where the inflammation is slight, or the system of the patient has been previously enfeebled, or phlebotomy has been carried to a considerable extent, local blood-letting by cups or leeches, is preferable, to further venesection, and often produces the most admirable effects—abating the inflammation, without further exhausting the patient, and frequently exciting the bowels into action beyond the use of any other means.

5. In the fifth and last place, I come to speak of the secretions. These are, generally, deranged or suppressed, in all the forms of violent intestinal disease, which we are considering. The restoration of them is an object not to be lost sight of and should, if possible, be accomplished before evacuation from the intestinal canal is attempted. For this purpose in part, we give opium and draw blood, as we can have little hope of restoring the suspended secretions, while great nervous irritation or inflammation is present. These morbid states being abated, we may act successfully upon the liver with large doses of calomel; and on the mucous membrane of the bowels, with saline and other gentle and hydrogogue cathartics, such as castor oil, sulphate of magnesia or soda, super tartrate of potash, and the tartrate of antimony. The secretory functions being in some degree restored, the remaining inflammation will be rapidly reduced, and copious purging readily effected, even by emollient injections.

Such are the various means by which the violent and painful affections, known under the names of colic, ileus, enteritis, intus-susception, and peritonitis, are to be met. The ultimate object in the treatment of the whole, is the restoration of the secretions and the peristaltic function, effects which we too often seek, in the first instance, to effect with power-

ful cathartics, when, in reality, they should, as a general rule, be the last items in the series of remedial agents.

It is not difficult to conceive of the observations which first suggested this practice. It must have been observed long ago, that when copious purgings took place in these maladies, the patient generally recovered—when it did not, he died. On this fact seems to have rested the rule which prescribed purgatives, with a resort to the more drastic and to larger doses, as the costiveness proved more obstinate,—the very reverse of what is safe and successful—for the greater the torpor of the bowels, the more intense is the irritation, nervous or vascular, which produces it, and, consequently, the greater the necessity of withholding those drastics, which might convert irritation into inflammation, or aggravate the latter, if it already existed.

But although it is easy to understand how our predecessors and the people, came, by confounding cause and effect, to believe that the purging was the main step in the cure; it is difficult to comprehend, how, in the present improved state of the science, this wrong association of ideas should continue, and exercise an influence, so decisive as it sometimes does, in the practice of what are called regular bred physicians. For the honor of the profession, I am happy to say, that the error is far from being universal, and that there are not a few of its members who direct their efforts upon the morbid condition, which produces the irregular or suspended peristaltic function, and by subduing it, enable the bowels, either spontaneously, or by the aid of gentle cathartics, to resume their action.

Having gone through an inquiry—far from a perfect one, I admit—into the pathology and therapeutics of several kindred affections of the intestinal canal, I propose now to say something in detail on one of them—that of the peritoneum occasioned by the action on its surface of foreign matters.

I need not remind the members of this society, of the general anatomical and physiological characters of this membrane. Closely enveloping the intestines, it is, at all times, quite contiguous to a surface, on which rest a great variety of gross matters, but still it has no capacity for bearing their impress; and

a copious experience has shown, that when any of them, or foreign matters of any other sort, enter its cavity, effects, both local and constitutional, of the most alarming kind, are immediately developed. A law of the organism has been violated, and the consequence, in general, is death. Doubtless, many have died from this cause, without its having been suspected; but cases enough are on record to furnish us with the symptomatology and morbid anatomy of these varieties of peritoneal disease; if not with the means of curing them. I shall proceed to enumerate the principal:

1. Extravasations of blood, from mechanical ruptures of the liver or spleen.

2. Discharges of pus, chiefly from abscesses of the liver.

3. Escape of the contents of the gravid uterus, by a rupture of that organ.

4. Effusions of urine, from the bursting of the bladder.

5. Escape of the contents of the stomach and bowels, from wounds, rupture, or ulceration.

6. Escape of the cystic bile from the same lesions of the gall bladder or its duct.

The effusion of blood into the cavity of the peritoneum, is rarely attended with much disturbance of its functions, but occasionally inflammation supervenes.

That of pus is not quite so harmless, as it is more difficult to absorb, and more irritating.

The escape of the contents of the gravid uterus, is, generally, fatal in a few hours. Vomiting takes place immediately, the heart fails in its powers, the patient becomes cold, and expires. In these cases we must add the effects of the laceration of the uterus, to those of the impress of the fœtus upon the peritoneum. Between the two, the patient commonly dies before inflammation is established.

The effusion of urine into the peritoneal sack, is a grave and fatal accident.

It has never happened to me to meet with a single case of any of these accidents; and I conclude, therefore, that they are of uncommon occurrence, and I shall dismiss them without further notice.

The effusion of the contents of the stomach, the bowels, and the gall bladder—particularly of the bowels—is a much more frequent event; and although, perhaps, always fatal, it deserves on various accounts, to fix the attention of the profession.

This effusion may be the consequence, 1, of wounds; 2, of contusions; 3, in reference to the gall bladder, of a puncture by the lancet when the organ is distended, simulates the form and other characters of a hepatic abscess; 4, in reference to the same organ, by distention and rupture; 5, of inflammation and softening of the stomach and bowels; 6, of ulceration and absorption.

When the bile only escapes into the cavity of the peritoneum, we may presume that the consequences are less urgent, than when the contents of the stomach or bowels, are effused, as they comprehend various crude matters, in connexion with that fluid.

The first effect of the entrance and spread of the contents of the primæviæ, or the gall bladder, upon the surface of the peritoneum, must be those of a gross foreign matter on a living surface, not previously subjected to that kind of action, nor indeed prepared to sustain it. Hence a series of morbid actions immediately commences. The symptoms which express the first part of this train are irritative; those which characterize the second, when the first do not prove fatal, are inflammatory. Let us enumerate the principal—excruciating pain, suddenly induced, and often beginning in a particular part; chilliness; vomiting; a small, feeble, and frequent pulse; paleness; alarm; muscular debility, and sometimes death in a few hours, from prostration of the vital functions; symptoms not unlike those from extensive burns and scalds, great lacerated wounds, or energetic acrid poisons received in large quantities into the stomach or a wound. If the patient should not fall an immediate victim to this irritation, other symptoms begin to blend themselves with those just enumerated. Costiveness is found to supervene, the abdomen begins to swell, and become painful on pressure; the patient complains of thirst; the pulse still continuing small and frequent, becomes wiry;

the diaphragm and abdominal muscles act with difficulty, giving rise to embarrassed respiration; the secretion of urine is suspended—symptoms which sufficiently indicate that inflammation has supervened upon irritation.

The foreign matter, however, still remaining in contact with the peritoneum, depresses the vital powers of the heart and brain, and however acute and rapidly disorganizing the inflammation may be, those governing organs never recover their tone, and the signs of debility and irritation in the sanguineous and nervous systems continue, and are blended with those of inflammation.

The proportions in which these two classes of symptoms occur in the same case, will depend on temperament, and the rigor of the constitution. When both favor the development of inflammation, it will take place speedily and extensively, though without the constitutional signs of an intense inflammatory condition; but, on the other hand, if the patient be of a nervous or phlegmatic temperament, or prone to hysterical irritation, the degree of inflammation, even when death does not take place for several days, will be inconsiderable. I have now before me, notes of four cases, occurring in this city within the last six years. The symptoms of all were nearly the same; in three of the patients the ravages of inflammation were found after death; in the other, none were *said* to be present, but still, it had existed, for the blood drawn 24 hours before death was sizy. The three former cases were from rupture of the bowels, the latter from that of the gall bladder; and here the question presents itself, whether the impress of the bile upon the peritoneum, is not of that kind which developes great nervous irritation, rather than inflammation. I am disposed to think that this is the fact, not only from the history of this case, compared with the others, but from the circumstance that in jaundice, when the elements of the bile are circulating throughout the system, the pulse is generally weak, the nervous system depressed and irritable, and inflammatory symptoms often absent, or feebly declared. Any acute inflammation of the liver, does not admit of the great loss of blood, like inflammation of the lungs or bowels, and I

can perceive no reason for the difference, but the absorption or retention of bile in the blood, in the former case; and its regular excretion, in the latter.

To whatever extent inflammation may be developed in these patients, and whether it terminate in gangrene, or the patient die from prostration and dyspnœa previously to that disorganization, the final symptoms are nearly, or identically, the same—great distention of the abdomen; cessation of pain; sinking of the heat of the surface; pallid, or purplish face; shrunken features; extreme difficulty of breathing; a wavering, and, at length, imperceptible pulse; clammy sweats; and a fixed position on the back.

Of the treatment of these cases, all of which, perhaps, are necessarily fatal, I need say but little. Could we be assured that n rupture had taken place, nothing need be attempted, but the exhibition of narcotics; but as this can seldom or never be known, it is our duty to prescribe according to the symptoms, on the theory of irritation, spasm, or inflammation from some common cause, for which I will refer to what has been already said, in a former part of this paper. In regard to blood-letting in these accidents, it was either injurious or useless in every case in which I have seen it employed. But when we do not know of the presence of foreign matter in the cavity of the peritoneum; and tension, pain, and tenderness of the abdomen, are present; what can justify the postponement of venesection? The lancet, moreover, is an instrument of diagnosis in these affections, for if there be extraneous matter upon the peritoneum, the pulse will sink, instead of rising, under its use.

The following observations of Dr. Graves and Dr. Stokes, on this subject, are so judicious, that I trust the Society will excuse me for reading them from Dr. Hays' abridgement of the Medico-Chirurgical Transactions, pp. 374–7.

"The foregoing cases of peritonitis from perforation of the intestine, having occurred within a short space of time, gave us a good opportunity of studying the disease. In most of them the perforation was owing to an ulceration of the mucous follicles of the ileum, preceded by a process of irritation for a greater or lesser space of time. Thus

we have seen the disease occurring in cases of gastric fever, and in diarrhœa, either idiopathic or induced by the use of purgative medicine; circumstances in which the mucous system is called into morbid action. Further, in all our cases, with but one exception, we have found near to the perforation, numerous marks of irritation in the muciparous system. Some of the follicles were enlarged, others softened down and forming ulcers, the bases of which were frequently formed by the serous membrane.

The symptoms in most of our cases were very similar. The sudden appearance of the peritonitic symptoms, and the rapid sinking of the powers of life, pointed out the occurrence of the perforation in the majority of cases. In fact, the peritonitis from perforation differs in this from idiopathic, that the patient is brought in a very short time to the last stage of the disease. An extent of injury takes place in an hour, which in the common peritonitis would perhaps take days to produce.

In one case, however, Pilkington, the disease was slow and latent. Here the only evidence of any abdominal affection was the recurrence of a slight tenderness of the epigastrium, which was in every instance removed by the application of a few leeches. We have heard of another case which occurred in this town, where, after a long fever without local symptoms, the intestine was found perforated in numerous places. In Pilkington's case, it is to be remarked, that no fecal matter escaped into the cavity of the peritoneum. The ulcerative process was evidently slow, and the abdominal exudation prevented the escape of the intestinal contents.

In the history, then, of this form of peritonitis, it is of the utmost importance to recollect, that it may take place in a very latent manner: fatal peritonitis may be going on unsuspected by the physician. This is to be particularly recollected when there is an epidemic tendency to affections of the intestinal tube, ulceration of the mucous follicles is more frequently observed to occur in a latent form than the peritonitis which results from it. Thus, we have known it to be found to a very great extent, and involving numerous patches of glands in the ileum of an athletic man killed by accident. During the late epidemic we only observed a single case of fever out of several thousand, where perforation took place, while during the last year this occurrence has been frequent in Dublin.

In the case of Michael Gill, the cessation of pain and tenderness was so remarkable, that we suspected the occurrence of gangrene; this was, however, not borne out by the dissection. Sudden cessation of pain and tenderness at the close of a violent abdominal inflammation, is then not a certain indication of gangrene: it may be produced by the depressed state of sensibility, brought about by the violence and extent of the disease.

We are of opinion, and we beg to call the attention of our readers to this circumstance, that the principle of treatment in these desperate cases is not yet fully understood. Are they to be treated like cases of idiopathic peritonitis? This is a question of the highest practical importance. We shall examine it briefly.

It appears to us that the antiphlogistic method is not so applicable

as in the simpler form of this disease. In the first place the perforation generally occurs at an advanced period of some other disease, when the constitution is less capable of supporting any loss of blood. In the second place, the disease, as we stated before, runs its course with such rapidity, that in a comparatively short space of time it brings the patient into the last stage. Now it is at this time the physician is most frequently called on to interfere, and as far as our observation has gone, the antiphlogistic plan only accelerates the fatal termination.

We know that nature makes an attempt to cure, and the question is, How are we to prolong life so that organization of the false membrane may take place? Is it by drawing blood after the peritonitis has come nearly to its last stage, when the powers of the nervous system are in the greatest state of depression? We apprehend not. Bleeding in peritonitis is useful in preventing the increase or extension of the disease; but in these cases the affection is *at once* extensive and severe.

These considerations make us submit that general, or even perhaps local bleeding, is a mistaken practice in this disease, and we are borne out by experience of cases somewhat similar, in recommending a different plan. In the first place, the antiphlogistic plan has failed in all our cases; this alone is an argument against it. Secondly, we know that certain cases of peritonitis, where the powers of life are greatly sunk, may recover under the exhibition of wine and opium, when blood-letting would be destructive. Thirdly, the exhibition of opium in large doses has succeeded in our hands in checking peritonitis, where the powers of the system were so much depressed as to make us avoid the atiphlogistic plan. In the case of Donegan, the use of this remedy was attended with the most singular benefit, and two cases have occurred to us where violent peritoneal inflammation succeeded to tapping for ascites in exhausted subjects; in both, although in a state apparently desperate, the disease was subdued by opium, without the use of either the lancet or leeches. *It is now nine years since this practice was introduced into the Meath Hospital.* In speaking of hepatic abscess, we have adduced another example of cure of general peritonitis, supervening on the opening of the abscess, attributable to the use of opium and wine; here the peritonitis was universal and its cure perfect, as was shewn by dissection.

We would then recommend the use of opium *in large doses* in cases of peritonitis from perforation. If the lancet is to be used, it should be only at the very outset of the disease, and even then its utility appears to us very problematical."

I shall now proceed to illustrate what has been advanced, on the symptoms, pathology, and morbid anatomy, of the peritoneal disease consequent on ruptures of the stomach, bowels, and gall bladder, by reading a number of selected and original cases.

I. Escape of the Contents of the Stomach.

1. *From Perforation.*—A gentleman, aged about 60, in the year 1825, had for a considerable time suffered from complaints in his stomach. He had occasional pain, but it was not severe; his more prominent symptoms were an intense feeling of Pyrosis, and occasional vomiting. He was often obliged to leave the table suddenly during meals from attacks of this kind, in which he chiefly brought up small quantities of an extremely acrid fluid. He became much emaciated, and had every appearance of extensive organic disease, though none could be discovered on examination. He required to be kept upon the most cautiously regulated diet; and after continuing for some months in a state from which he was not expected to recover, he gradually got into his former good health, and his stomach entirely recovered its healthy functions. He had at various times, however, slight threatenings of his former symptoms, and required to live with great caution; but he was full in flesh, and his general health was excellent. About a fortnight before his death, he had one of those slighter attacks, which affected him chiefly with a distressing feeling of Pyrosis, impaired appetite, and occasional vomiting. On account of these symptoms he was keeping the house, though able to attend to the affairs of an extensive business, until Saturday evening, 3d February, 1827, when he was suddenly seized with excruciating pain in the pit of the stomach, accompanied by some vomiting, coldness of the body, and a small frequent pulse. From the moment of this attack, nothing that was done afforded the least relief. He continued in the most violent and unceasing pain through the night and through the following day; The whole abdomen became distended and tender, with sinking of the vital powers, and he died on Sunday night, about 30 hours after the attack.

Inspection.—On the posterior surface of the stomach near the Pyloric extremity, there was a space rather larger than a shilling, where the substance of the stomach was entirely destroyed; but the margin of the opening adhered all around very closely to the surface of the liver, which thus preserved the continuity of the part. Below this portion, and very near the Pylorus, there was an ulcer on the mucous membrane, smaller than a sixpence, and through this a perforation of the coats had taken place of such an extent as would have transmitted a full sized quill. Through this opening the contents of the stomach had escaped into the cavity of the peritoneum, where there were exhibited the usual marks of extensive but recent peritonitis. Except the two spots now referred to, the stomach was perfectly healthy.

Abercrombie, pp. 58–9.

2. *From a fissure of the Pylorus.**—In the afternoon of the 14th of April, 1828, I was called to see E. P., aged 38 years, by occupation a cordwainer. He was said to labor under colic; and to have been

* This and another case have been already published in this Journal, and it is not without some misgivings that the *Editor* here inserts them. His apology is, that they were inserted in early numbers of the work, that it has acquired many new subscribers, and that the paper read before the Society would be defective without them.

attacked suddenly and violently. I found him sitting up, with a pale and cool skin, a feeble pulse, beating not more than 48 strokes in a minute, and complaining of excruciating spasmodic pain in the abdomen, the paroxysms of which were followed by incomplete fainting fits. Regarding it as an attack of flatulent colic, I, immediately directed a hot foot-bath; and began the preparation of a large dose of opium and calomel. Before it was ready for admnistration, he was seized with retchings, and drank a glass of tepid water. Slight vomiting occurred, with agonizing pain, which was followed by a degree of syncope that bordered on dissolution. As soon as he revived the portion of opium and calomel was administered, together with a quantity of paregoric. Stimulating, cathartic enemas were then employed; but they produced no fæcal evacuation, and, indeed, were themselves but partially discharged. About 10 o'clock I left him for the night, in the care of my young friend Dr. McDowell. The Calomel and opium were repeated; laudanum was liberally administered; a dose of castor oil was given, and the injections were repeated, in the course of the night; but nothing more than palliation followed. The next morning I found him thirsty, in pain and drowsy, with his abdomen distended, especially *in epigastrio*, and tender on pressure. His pulse was feeble, exceedingly variable in its rythm, at no time more than 60 in a minute, and frequently down to 46. It was obvious that peritonitis had supervened, and I bled him. The blood was sizy; but his pulse fell instead of rising in its energies; no alvine evacuation ensued; nor were the pain and swelling diminished. Calomel was now administered in large and repeated portions, with occasional doses of sulphate of magnesia and castor oil; the injections were repeated, and the abdomen was blistered. In the evening the symptoms were mitigated, and a small quantity of feculent matter was discharged. During the night a gentle salivation came on; he rested badly; had occasional retchings, in which he threw up but little; and the next morning was worse, than I had left him in the evening. The epigastric tension, and soreness on pressure, especially had increased. The pulse remained nearly the same in frequency, but was still weaker and smaller. The case now gave me equal embarrassment and apprehension; for it exhibited unequivocal evidences of abdominal inflammation, combined with an exhaustion of the vital powers, which was as manifestly occasioned by something else, than the peritonitis, which had been developed. I could not doubt that a structural lesion existed; but of what kind it seemed impossible to determine. Examination was made for hernia—ventral, inguinal and femoral—but none existed; and I was led to conjecture that intussusceptio had taken place. I desired a consultation, and, in the afternoon, Dr. Pierson was called in. He was struck with the obvious signs of peritoneal inflammation; and the patient was again bled more copious than before. The pulse sunk during the operation, and the blood was not sizy. The abdominal tumefaction and agony increased; his pulse became imperceptible, and toward morning he expired; about 60 hours from the commencement of the attack.

Examination of the Abdomen.—Leave being granted to inspect the

abdomen, it was done the evening after his death. The distention was very considerable, and upon penetrating the cavity of the peritoneum, a great quantity of offensive gas escaped. On making an extended crucial incision, that membrane was found to be inflamed, and to have thrown out a quantity of coagulable lymph, with some admixture of pus. It was slightly agglutinated to the bowels; and these were extensively adherent to each other. The whole were immersed in a turbid liquor, of a yellowish colour, which emitted the smell of turpentine, and exhibited no inconsiderable quantity of oil floating on its surface. A large worm (lumbricoides) was crawling over the bowels. After sponging out between two and three quarts of fluid, (the quantity extravasated) we found the outer tunic of all the anterior and upper convolutions of the intestines, great and small, in a state of inflammation. The omentum was in the same condition. The spleen was large, tender and so much inflamed, that its peritoneal covering peeled off. The liver was but little affected. The gall bladder was distended with healthy bile. The urinary bladder was empty and uninflamed. On examining the intestines, from the rectum to the stomach, no lesion was found, till we reached the pylorus; where the cause of all the mischief was discovered. *It was ruptured more than half way round, and the fissure exhibited the appearance of a clean cut.* Through this, the contents of the stomach and duodenum had escaped into the cavity of the peritoneum. On slitting open the bowels and stomach, the mucous membrane of the former was healthy, and the pyloric extremity of the stomach was equally healthy, on both sides. The mucous coat of its great extremity was considerably, but not deeply inflamed. No ulceration existed any where; nor had any part suffered gangrene. The closest inspection of the pylorus, did not enable us to detect any organic change, that could account for its laceration.—*West. Journal, vol. 2, pp.* 127-9

II. Escape of the Contents of the Small Intestines.

From Perforation of the Ileum.—A girl, aged 15, (12th May, 1818) had fever, with pneumonic symptoms; was bled with relief; the fever subsided gradually, and on the 19th she was considered as well. On the 20th, at night, she complained of some pain of the belly, which soon went off, and through the night she felt no uneasiness. On the 21st, had violent pain and tenderness of the abdomen, with some vomiting; pulse frequent. Took an opiate, and afterwards some purgative medicine; the vomiting subsided after the opiate; the pain also was much alleviated, and was only complained of upon pressure. The purgative did not operate during the day, but operated freely in the night four or five times. I saw her for the first time on the morning of the 22d, and found her moribund; the pulse not to be counted from its frequency; features collapsed; belly tympanitic. She died in less than an hour after the visit.

Inspection.—On the surface of the bowels on many places, especially on the ileum, there was peritonitis with deposition of false membrane. On the inner surface of the ileum, near the caput coli, there was an inflamed portion, in the centre of which, there was a

white spot the size of a shilling; and, in the centre of this spot, a round perforating aperture, which transmitted a quill; the edges of it were rounded, and a little thickened. Much fluid feces and gas had escaped into the cavity of the peritoneum, and the bowels were not distended; there were in some places a few livid spots, but no gangrene.—*Abercrombie, p.* 179.

2. *From Perforation of the Ileum.*—Patrick Kane, aged 46, a laborer, was admitted on the 2d of October, 1828, laboring under severe symptoms of peritonitis. About a year previously this man had contracted ague, for which he took bark, which stopped the rigors, but he never recovered his appetite, and labored under continual thirst, with occasional soreness in the region of the stomach, and yellow slimy dejections. Twelve days before his admission he was exposed to cold, when he was attacked with shivering, followed by total anorexia, vomiting, and severe pain across the umbilical region. These symptoms were succeeded by a bilious diarrhœa, which continued for ten days, when suddenly the pain became intense, the diarrhœa ceased, and he found himself unable to pass his urine; the pain was general over the abdomen, and was particularly severe in the epigastric and umbilical regions; swelling of the abdomen appeared immediately after. The following was the report of his state on the day of admission. The belly is swelled, tympanitic, and extremely tender; pulse 88, small and compressible; respirations hurried, with occasional moaning. He passed his urine to-day, and has had some vomiting; no stool. Yesterday had occasional singultus; *leeches were applied to the abdomen; tobacco and purgative injections.*

October 2d. Pulse somewhat fuller; pain rather less; no evacuation by stool; copious vomiting of a brown fluid; venesection to ʒxvi. three grains of calomel every second hour.

3d. A copious purging came on at midnight of natural colored fæces; vomiting continued. There is no pain or tenderness of the belly; pulse imperceptible; countenance sunk; singultus. Death at noon.

Dissection, fourteen hours after Death.—Belly tense and tympanitic. On opening the peritoneal cavity a quantity of fœtid air escaped, and the sac was found to contain much yellow fluid, mixed with fecal matter and flakes of lymph. The peritoneum covering the liver and abdominal parietes, presented a thin coating of false membrane, easily detached by the scalpel. The small intestines were of a livid color, and covered in many places by coagulable lymph; the lower portion of the ileum adhered strongly to the bladder by very consistent lymph. On its being detached, we found a large perforation of the intestine corresponding to the fundus of the bladder; it was caused by a follicular ulceration occurring about twelve inches from the ilio-cæcal valve. In this part of the gut we found numerous ulcerations of the same kind, of various depths, but no other perforation.

Med. Chir. Trans. pp. 368–9.

3. *From Laceration of the Jejunum.*—December 1st, 1827—John McDonald, coachman of Mr. P. B. of this city, aged about 22 years, received a kick from one of the carriage horses, standing in the stable. The accident happened at 3 o'clock, P. M. and the unfortunate

man expired between 8 and 9 o'clock in the evening of the next day, or in about 30 hours. From the complaints and statements of the patient, the shod foot of the horse struck him on the left and front part of the abdomen, between the diaphragm and the crest of the ileum; but he could not indicate the spot, and no appearance of the integuments enabled me to identify it. He was able to walk from the stable to the house after the accident; but immediately laid down and complained excessively of abdominal pain, and exhibited a kind of hysterical restlessness. His pulse was considerably increased in frequency, but retained as much force and fulness, as can be associated with that condition. As he had not dined, I presumed his stomach to be comparatively empty; and, therefore, not liable to rupture; the constitutional symptoms, moreover, seemed not to warrant the opinion that such an accident had occurred; and viewing it, though with some doubts, as a case of nervous irritation, I directed 30 drops of laudanum internally, with towels dipped in *hot* water to the abdomen.

In visiting him at night, I found him still in great pain and inquietude. His pulse nearly the same; and his abdomen considerably swelled, tense, and painful to the touch throughout its whole extent. Still more perplexed than before, as to the pathology of his disease, I took twenty ounces of blood. It produced a strong disposition to syncope, but no alleviation of his distress. Ordering two ounces of sulphate of Magnesia in divided doses, I left him for the night.

Next morning, I found him much worse. He had slept none; the pain continued; he had vomited several times; no fæcal or urinary evacuation had taken place, though being thirsty he had drank copiously all night; the abdominal swelling had increased; and his pulse, more frequent than in the evening, had lost much of its volume and firmness. Having seen great benefit from large doses of calomel in peritoneal inflammation attended with feeble pulse, and incapable of comprehending more of the case, than that inflammation was present, I administered 30 grains of calomel.

At 12 o'clock all the symptoms were greatly aggravated. The nurse reported that he had discharged nearly half a pint of urine tinctured with blood; but as he complained of great pain above the symphysis pubis, labored under priapism, and had drank freely, I determined on introducing the catheter; not a drop of urine or blood, however, was evacuated.

I was now convinced that a blood vessel had been ruptured, and internal hæmorrhage existed; or that a rupture of some portion of the alimentary canal had happened; or that extensive inflammation had been suddenly developed, and would speedily terminate in gangrene. Regarding either of the two former læsions as almost inevitably fatal, I thought only of prescribing for the third; and censuring myself for not having drawn blood more copiously in the night, I resolved to make, at least, nominal amends for the supposed omission, and opened a vein. Not more than five or six ounces would flow, and at the end of the operation, his pulse was nearly imperceptible, and his skin leaden, clammy and cool. Directed ol. recini ʒij, cathartic enemata, a blister to the abdomen, and a hot foot-bath.

At seven in the evening, his pulse was imperceptible at the wrist, and extremely feeble in the carotid; his respiration greatly embarrassed, and his mind wandering; all the symptoms, in short, portended approaching dissolution, though he was still able to move about in bed, and sit up without support. In less than two hours afterwards he expired.

Appearances on Dissection.—Permission to examine the body having been granted, the dissection was executed the next day at 11 o'clock, fifteen hours after death.

The abdomen was greatly distended. The skin exhibited no signs or effects of contusion. We were struck with the induration (almost corneous) where the blister had been applied, although it seemed, while he was alive, to have produced no effect. This change of texture, which, doubtless, had no connexion with his malady, would seem not to be of easy explanation.

On dissecting up the integuments, we found an extravasation of blood upon the external oblique muscle of the left side, and between that and the internal, near the junction of their tendinons and fleshy portions betwixt the anterior superior spinous process of the ileum and the umbilicus. This determined the spot which had received the blow. On puncturing the peritoneoum, a great quantity of gas escaped. On laying it open, there flowed and was sponged out, more than two quarts of turbid fæculent yellowish fluid, without any blood. Indeed, no hæmorrhage had existed; but that a rupture of the stomach or bowels had taken place, was sufficiently obvious. The peritoneum, both of the parieties and intestines, every where exhibited signs of inflammation. The omentum had been much inflamed. The intestines, generally, were agglutinated together, and to the walls of the abdomen. There had, indeed, been universal peritonitis, but death occurred before it had terminated in any fatal organic læsion. The inferior or pelvic portion, exhibited signs of greater inflammation than the epigastric portions, doubtless because the extravasated contents of the intestines, had gravitated to the lower part of the abdomen. The bladder was quite empty, much contracted and inflamed in its outer coat.

Beginning with the rectum, I examined the bowels upwards, to find the rupture. All was sound till I came within eighteen inches of the duodenum, where I found the jejunum cut transversely one-third across. The edges of the wound were lacerated, thickened, somewhat rolled back, and adherent to the outer coat of the bowel upon which they were turned. In another place, close to this, the intestine was nearly cut through. The mucous membrane was but little inflamed. The peritoneal covering of the liver, gall, bladder and spleen, was slightly inflamed. The cellular substance over the second and third lumbar vertebræ was injected with blood, and much thickened. The left psoas muscle was the seat of considerable extravasation—it was swelled, tender, and had some of its fibres torn. It was sufficiently obvious, that the bowels had been forcibly pressed, by some part of the shoe of the horse, against the lumbar vertebræ and thus lacerated. Such an injury could not, *of itself*, occasion speedy death, and the early and fatal termination of this case, can only be referred

to the impress on the peritoneum, of the contents of the alimentary canal, escaping through the aperture in the jejunum.—*Western Journal, vol.* 1, *pp.* 550–4.

III. Escape of the Contents of the Great Intestine.

1. *From Perforation of the Colon.*—Michael Donegan, aged 12, was admited on the 22d of September, 1829. It appeared that ten days previously to his admission, he was attacked with severe pain in the abdomen, which increased, and was followed by other symptoms of enteritis. These were not combated by any proper remedies for a week, when he was first seen by Mr. Pakenham, who found him in a state of great exhaustion. Pulse 58, sharp and intermittent; countenance sunk; universal tenderness of the abdomen; frequent vomiting; diarrhœa and tenesmus: calomel and opium were exhibited, the belly was leeched, and the patient was placed in a warm bath. Next day, the pulse being stronger, he was bled; the blood was not inflammatory. He was admitted on the following day.

His countenance was then collapsed, anxious, and expressive of abdominal suffering; the extremities cold, and the pulse hardly perceptible. The respiration was hurried, and the abdomen swelled and exquisitely painful. Constant tenesmus.

Under all these circumstances, we determined on trying the effect of free doses of opium, having witnessed its good effects in the case of Clare, already detailed. Large doses of the black drop were given every second hour, and mercurial frictions used over the belly. On the next day a great improvement was perceptible: the pulse was full and soft; the extremities were warm; the face had lost the hipprocratic expression; he could bear pressure on the abdomen, and the tenesmus had ceased. The patient, who, on the day before, was nearly insensible to external objects, expressed great relief and confidence of recovery. The same treatment was persevered in for the next twenty-four hours, and on the following day ***all symptoms of peritonitis had completely subsided.*** The belly felt natural; there was no tenderness; the pulse was good, and the patient declared himself well. We now unfortunately changed our plan of treatment; the opium was omitted, and a gentle saline laxative exhibited; this produced four evacuations, followed by a return of all the symptoms of abdominal inflammation, under which he speedily sunk.

Dissection, ten hours after death.—The abdomen was distended and tympanitic; the intestines were every where agglutinated together, and adherent to the parietal peritoneum, except in the left iliac fossa, where a quantity of yellow puriform matter was collected. Small quantities of the same fluid were found between several of the intestinal folds, circumscribed by the effusion of lymph; general livid redness of the serous membrane. On detaching the caput coli from the peritoneum, lining the right iliac fossa, a small perforation of the gut was discovered by the escape of the contents of the intestine in a jet. The serous membrane lining the iliac fossa was found of a bluish color, and softened for a space of three square inches: this change was also observed in the lower portion of the ileum. On opening the gas-

tro intestinal tube nothing remarkable was observed until within the last twelve inches of its termination, where the mucous membrane was softened, and dark colored. The perforation of the large intestine, before mentioned, was apparently the result of an apthous ulceration, as no glandular enlargement or ulceration could be detected round it. Here, however the mucous membrane was softened, and of a deep red color, while the remaining portion of the intestine was healthy.

2. *From Perforation of the Rectum.**—On Friday night, Jan. 10, 1834, at 11 o'clock, I was requested to visit Mr. J. M. Lee, of this city. On my arrival I found him in great agony, and the anxious expression of his countenance convinced me that he was extremely ill. He informed me, that he had been somewhat costive; and had, about 4 o'clock that afternoon, endeavored to procure an evacuation from his bowels, and during the effort, *he had ruptured some part.* He said he was conscious of it at the moment, and could not be mistaken—the same declaration he had already made several times to his family, whom he forbade to send for a physician, because he felt convinced that it would be unavailing.

He had been exposed at a late fire, and had taken cold, but his general health was better than it had been for months. I knew that his habits had been intemperate, and that he had labored under a violent attack during the first invasion of the cholera. I have since learned, that he had, until the last few weeks, been troubled with diarrhœa, but not to an extent, which, in his estimation, rendered a resort to medical aid necessary. Some few days before I saw him, it had given way to a torpid condition of the bowels. He had, in the early part of the day on which he was taken, been at work; and in conversation with his friends, he had frequently exulted in the prospect of returning health and vigor.

Immediately on the occurrence of what he termed a 'rupture,' the most excruciating pain commenced in the hypogastric region, and rapidly extended to the whole abdomen. The great rigidity of the muscles prevented distention; but the entire surface, as well as the deep seated parts, was so exquisitely sensitive, that the slightest touch produced an agony which made him cry out—indeed, his respiration was constantly interrupted by groans and screams.

* Communicated by John F. Henry, M. D.

My first impression, on hearing his statement, was, that he labored under strangulated hernia; and this opinion was strengthened by his peculiarly anxious countenance, and the soft and compressible character of his pulse; as well as by the fact, that two hours after the occurrence to which he ascribed his illness, he had vomited, and fainted. On examination, however, I was convinced that no such condition existed; and the best diagnosis I could form, was, that the peritoneum was in a high state of inflammation; but of the cause which could so suddenly develope it, I was unable to form any rational conjecture.

Under this view of his case, and with the hope that the inflammation might be prevented from terminating in mortification, to which it seemed to be rapidly tending, I ventured to take blood freely, although the pulse was but little accelerated, and in point of strength and volume, was below the standard of health. I prescribed, also, 40 grs. of calomel and two of opium, and hoped I would find him improved in the morning. Such, however, was far from being the result. On visiting him at that time, the pain was undiminished; he had slept none: and no evacuations from the bowels had taken place, notwithstanding the administration of the most stimulating enemata, during the latter part of the night. I then resorted to castor oil and epsom salts, alternately, with a continuance of the enemata. I drew blood a second time; and applied a blister, ten inches square, to the abdomen His stomach rejecting the oil and salts, and the injections failing to excite the bowels, I resumed the use of the calomel and opium; but it did not even *mitigate* his sufferings, although the proportion of the narcotic was large. On the next morning, about 11 o'clock, he expired having lived 43 hours from the commencement of his illness.

Dissection.—The case being one of great interest, as well as of much obscurity, I was anxious to ascertain the cause of its sudden accession and rapid progress, and obtained permission to examine the viscera of the abdomen. This was done 24 hours after death, and disclosed the following morbid appearances:

On opening the cavity of the abdomen, the peritoneal lining was found in a state of high inflammation, bordering on sphacelation. The greater omentum was highly inflamed, and reached down into the cavity of the pelvis, where it was attached by adhesions which seemed recent, as they were easily broken by the fingers. Appearances of chronic disease were visible, in the thickened and knobulated condition of this membrane. On lifting it up the whole peritoneal surface of the bowels was found inflamed, and covered with a purulent fluid, which gushed out whenever the adhesions which bound together all the convolutions of the bowels were broken. These adhesions extended throughout the whole course of the intestines, from the duodenum to the rectum; so that no one portion of the alimentary canal was loose and floating. The quantity of false membrane and pus was very great. On separating the bowels, spots of a dark livid color were found throughout their whole extent; but no part was in a state of actual mortification. There was, also, more than a quart, by computation, of an exceedingly offensive and feculent fluid; and on tracing the bowel, from the stomach to the rectum, an aperture was discovered in the anterior part of the superior portion of this bowel, large enough to admit the point of the finger. The mucous coat projected outward, as if its edges had been pressed through the opening by the escape of fluid or flatus. This, of course, was the cause of the inflammation and his speedy death; and explained, at once, the whole phenomena of this disease.

I regret that, from indisposition, I was compelled to desist from farther examination, as much light might have been thrown on the cause of this lesion, by examining the state of the mucous membrane. My conjecture at the time, was, that an ulcer of this membrane had gone on extending and deepening its cavity, until having almost destroyed the continuity of the parieties, the strong pressure of feculent matter and flatus, in the effort to procure an evacuation, had ruptured the remaining portion, and produced a communication between the cavity of the bowel and that of the peritoneum.

IV. Escape of the Contents of the Gall Bladder.

1. *Softening of the Cystic Duct without Perforation.*

Before citing instances of bilious effusion, I will read to the Society the following case, from Dr. Abercrombie, page 384, as illustrating the manner in which perforations of the cystic duct *may* occur.

A lady, aged 60, had been for several years liable to attacks of acute pain in the right hypochondriac region, which generally continued in great severity for a few hours, and then subsided suddenly. On Wednesdsy, 14th January, 1824, she was seized with pain corresponding to her former attacks, but which did not subside as usual. It continued through the night, accompanied by frequent vomiting and constitutional disturbance. On the 15th, there was fever, with frequent vomiting and obstinate costiveness, and the pain was more extended,—being referred to a considerable space on the right side of the abdomen. Belly tense and rather tumid. The case had assumed the character of ileus, and all the usual means were employed with little relief.—16th. There was some discharge from the bowels after a tobacco injection, but it was very scanty. Severe pain continued, with every expression of intense suffering. Her strength sunk, and she died on the morning of the 17th.

Inspection..—Every part of the intestinal canal was perfectly healthy, except the upper part of the duodenum, where there was considerable appearance of inflammation, with remarkable softening, so that it was very easily torn. A large irregular calculus was found sticking in the ductus communis, and the parts were so *softened that it came through the side of the duct when it was very slightly handled.* In the texture behind the duodenum there was considerable appearance of inflammation. No morbid appearance could be detected in any other organ.—*Abercrombie, pp.* 384–5.

It is evident, that if this patient had lived a little longer, the gall duct would have been perforated.

2. *Remarks on the escape of the Bile from Rupture and from Ulceration of the Gall Bladder or one of its Ducts.*

The immediate effect of this accident is rapid peritonitis, fatal in eighteen or twenty-four hours. The symptoms preceding it will depend upon its cause, and consequently may be either very obscure, or such as indicate great distention of the gall bladder, with obstruction of the bile in its passage out of it. The causes of the affection are chiefly referable to two classes.

(1st.) Obstruction of the common duct. This may take place rapidly by adhesive inflammation, or more slowly by gradual obliteration. In the former case the symptoms are rapid, as in a man mentioned by Andral, who had acute pain followed by jaundice, and a pyriform swelling rising up from under the margin of the ribs. On the 5th day

he was suddenly attacked with peritonitis, and died in twenty-four hours. The ductus communis was found much contracted, and at one place obliterated. The gall bladder and the hepatic and cystic ducts bore marks of having been much distended; the rupture had taken place in the hepatic duct, and much bile was found in the peritoneal cavity. In another the symptoms of obstruction to the passage of the bile had been going on for between two and three months before the fatal attack, and in this case both the cystic and common ducts were found much contracted.

2d. Perforation of the coats of the gall bladder by ulceration. A man mentioned in the Noveau Journal de Medecine for 1821, had been affected for more than a month with pains in the abdomen and fever, which had various remissions and aggravations. On the 37th day of the disease, he was suddenly seized with symptoms of the most violent peritonitis, and died on the following morning after suffering inexpressible agony. On inspection, there were found marks of most extensive peritonitis. The inner surface of the gall bladder presented numerous small circular ulcers from one to three lines in diameter; two of them had entirely perforated its coats, so as to allow the escape of the bile into the peritoneal cavity.—*Abercrombie, pp.* 385 6.

3. *From Rupture.**—On Thursday, of the first week of September, 1829, I was called in haste to see Mr. Russel Bottsford. He complained of severe pain in the right hypochondriac region, attended with paroxysms apparently spasmodic, which caused him to draw down his breast towards the pelvis, and press his hands upon the upper part of the abdomen. He had vomited once or twice before I saw him.

He said he bad been in a profuse perspiration the evening before, and had retired to bed early, in a very open counting room. The weather had become suddenly cold during the night, and he awoke about 4 o'clock in great pain and distress.

I had seen him in several attacks of a very similar kind, during the two years preceding, which had yielded to the prompt administration of an anodyne and cathartic dose, together with hot applications to the epigastrium.

It was now 9 o'clock, A. M. He had been very chilly before I saw him, but a considerable reaction had taken place; his pulse was somewhat tense, and his manner agitated, showing much disturbance in the nervous functions. He was immediately bled, but the blood did not run freely, so that not more than 10 ounces was taken. I then administered calo-

* Communicated by the attending Physician.

mel and pulvis Doveri, each 15 grs.; had a large mustard poultice applied over the abdomen, and ordered the best means to be used to encourage perspiration.

At 2 P. M. the pain was somewhat abated, though still attended with paroxysms of considerable severity. He was now bled freely, a large blister was applied over the seat of the distress, and more active cathartics were administered.

At candlelight I saw him again. The pain was less severe; only one slight operation from the bowels; skin dry.

About 10 o'clock the same evening, he had severe paroxysms of pain, attended with violent vomiting, after which the pain left him. During this violent paroxysm Dr. W. was called in; but as the pain had abated no new treatment was ordered.

On Friday he was comparatively easy; had two small evacuations from the bowels; skin dry, but no fever; and he considered himself as getting well.

On Saturday morning he told me he was better, and should be out in a day or two. His pulse was feeble, and he complained of being weak, the skin was cool, and softer than yesterday.

About noon he grew worse. Dr. W. saw him, with me, soon afterwards. He was evidently sinking; the pulse was 120, weak and small; the extremities livid, and inclining to be cold; and the eyes glaring. Ordered him to be sponged freely with the nitro-muriatic wash as hot as could be borne, and sinapisms to the extremities.

He passed a restless night, complained of great oppression; an undefinable feeling in the epigastrium, and great heat along the spine. His mental faculties were not impaired, and he apprehended no danger until he was informed he was dying. He expired Sabbath morning, about 11 o'clock, 78 hours after the attack.

Dissection.—Leave being obtained, the body was examined by Dr. C. and myself, 24 hours after death.

On opening the abdomen, a large quantity of ropy greenish-yellow fluid, with whitish, curdly looking matter floating in it, was diffused throughout its cavity.

The bowels were greatly distended; no recent inflamma-

tion or increased vascularity appeared in any of the abdominal viscera.

There were marks of his having suffered a great degree of inflammation in the hepatic region, at some former time: viz: the liver adhered firmly to the abdominal parieties throughout its whole extent, the anterior edge of its lower surface adhered closely to all the organs on which it naturally rests. The peritoneum was greatly thickened, and many adhesions existed between the organs in the neighborhood of the liver.

On detaching that organ carefully from its natural adhesions, the gall bladder (which seemed to have been unusually large) was flaccid, with a rent in its side, into which the finger might be thrust. It contained a calculus three-eighths of an inch in diameter, with a very rough (mulberry) surface. No other appearance of disease was discovered.

Should it be asked, "at what period I suppose the rupture to have taken place?" I would say, in answer, that I believe it to have happened on Thursday night, in the violent fit of vomiting which then occurred, and after which the pain or spasm left him; about 18 hours from the first attack, and 60 hours before death.

4. *From Perforation of the Cystic Duct.—Case of John B. Brooke, of Cincinnati.* This gentleman was about 34 years of age. He had experienced more than one attack of torpor and deranged functional action of the liver. His temperament was nervous, and his state of mind, under all kinds of indisposition, alarmed and hysterical.

On the evening of Saturday, January 18, 1834, being in his usual state of health, after having taken no dinner, and eaten an ordinary supper, he was seized with acute pain in the right side, about the region of the liver. The pain abated in a short time, and he walked out; but about 12 o'clock at night it returned with excruciating violence, and was then chiefly in the lower or hypogastric portions of the abdomen. Dr. Robert Moorhead was then called in, and soon afterwards Dr. John Moorhead. The next morning he was costive, and though easier, by no means free from pain, and continued in that

way (with an occasional exacerbation) for the next 24 hours. His abdomen was swollen, and tender on pressure, and he was thirsty. On Monday morning, having slept none through the night, he was still worse. In moving he felt something give way, as he thought, within him. His costiveness still continued. About noon he began to vomit. In the evening, Dr. Eberle was called into consultation. On Tuesday morning these gentlemen were *replaced* by Dr. Richards and myself. We first saw the patient about 9 o'clock. He was then using a mixture of castor oil, spirits of turpentine, and croton oil; with stimulating injections every half hour or oftener, which had already been repeated a great number of times. A large blistering plaster covered the anterior part of the abdomen. He had taken assafœtida, cathartic pills, infusion of senna, pills of croton oil; and was then on the use of the mixture just mentioned. On the evening before, he had been bled to about 14 ounces. The blood exhibited considerable siziness.

When we first saw him, the following were the symptoms: sleepiness from the beginning; countenance, haggard and pale; dyspnœa, very great; posture, on his back, and unable to turn on either side; abdomen, from the epigastrium to the pelvis, swollen, tense, still extremely painful, especially near the symphysis pubis, and exquisitely sensible to pressure; constipation, still remaining; heat of the extremities reduced; pulse 124, weak and small; secretion of urine suspended, as was ascertained by introducing the catheter, when the bladder was found empty. The whole of these symptoms continued except the pain and soreness, which, gradually abating, ceased altogether by 3 or 4 in the afternoon, when he declared himself better, and said if he could get some sleep he would soon be well. But his pulse was then imperceptible, and he expired between 5 and 6, without delirium.

Soon after being called in, I was struck with the resemblance of the symptoms of this patient to those of E. P. and J. McD. just read to the Society from the Western Journal, and mentioned it to Dr. Richards, and some of the friends of the patient, inquiring of the latter, whether Mr. B. on the day

previous to his attack, had suffered any violence, or made any great and sudden effort, that could have ruptured the stomach or bowels, but was assured he had not.

On analysing the present and the past symptoms, we came to the conclusion, that it was a case of peritonitis, about to terminate in gangrene, and expressed this opinion to the friends.

There seemed little or no prospect of saving him, but as he and his family insisted on our attempting something, we gave him two or three doses of calomel in quick succession, and resorted to cupping. This was undertaken, first, over the epigastrium, where the blister had acted as a rubefacient, and afterwards on the sound skin of the flank. His sensibility, both to the scarification and the cups, was acute, but the skin was so exsanguinous that very little blood was obtained. During the operation, however, and subsequently, he had several small and liquid alvine evacuations, but they were not followed by any relief. By discontinuing all kinds of drink, his vomiting ceased. Warm fomentations were applied to the abdomen, and in the afternoon he took two moderate doses of laudanum. This completed the treatment of the case.

Post-mortem Examination.—The next morning the Doctors Moorhead obtained permission to examine the body. Dr. Richards and myself were not requested to attend; but a friend of that gentleman invited him in. The following were the appearances, as reported by Dr. Smith:

"The cavity of the abdomen contained a quart or more of a ropy gelatinous fluid, of a bilious color. The peritoneum was generally tinged with a bilious deposition; and where it covers the right lobe of the liver and the diaphragm, the deposition was thicker than elsewhere. Over these parts the fluid seems to have been retained some time before its general effusion throughout the abdominal cavity. There were some slight adhesions, which were not of recent formation, between the upper edge of the omentum and the liver, just over the cystic and hepatic ducts. The gall bladder was contracted, and there was near its neck, a small opening, arising from ulcerations, probably caused by a rough gall-stone, which occupied the ulcerated cavity.

The descending arch of the colon, the sigmoid flexture, and the rectum, were contracted to less than an inch in diameter. All the other parts of the colon, also the ileum, and part of the jejunum, were very

much distended, and were filled with air.—Several coils of the ileum, next the caput cœcum, of SOME FEET IN LENGTH, were lying deep in the pelvis, between the bladder and rectum. The mucous coat of the stomach and bowels presented no unnatural appearance. There were no traces of inflammation hyperæmia, or mortification. The other viscera presented a natural aspect. No further examination was made."

Such were the symptoms, and morbid appearances, in the case of Mr. Brooke, who seems, manifestly, to have died from the impress of bile upon his peritoneum. No treatment did him any good, and no treatment could have saved him. I shall trouble the Society no further.

Appendix.

I propose to add something to the paper read before the Society. It cannot, I think, be found uninteresting to the readers of the Journal, as it will involve several questions in pathology and morbid anatomy.

A few days after the death of Mr. Brooke, the Doctors Moorhead brought his case before the public, in the common newspapers, and in hand-bills distributed through the city and among the students of the Medical College of Ohio. In their several publications, which abounded in libels on my professional and private character, and in a manuscript communication to the Cincinnati Medical Society, where, *subsequently* to their first publication, I gave an account of Mr. Brooke's case, they presented the following points:

1. That Mr. Brooke died from *Colic*, and not from the escape of bile into the cavity of the peritoneum, and that inflation of the bowels is evidence of previous colic.
2. That during his illness, they considered that his colic was dependent on intussusception or on displacement of the bowel.
3. That three feet of the ileum was lodged in the pelvis, between the bladder and rectum, both of which were empty, and that this *displacement* was the cause of the colic which produced his death.
4. That a rupture of the gall bladder is not necessarily fatal; and that a case is on record, in a book of high authority, which was cured.
5. That the rupture in Mr. Brooke's case did not take place till the day preceding his death.
6. That the cupping, practised in the forenoon of the day on which he died, at sun-down, was a cruel and unjustifiable measure.
7. That there was no inflammation.

I am almost ashamed to fill up the pages of this Journal with comments on these different propositions; but the distant reader will perhaps excuse me, when he is informed, that one of the gentlemen has for many years been at the head of the Medical College of Ohio, and regarded by its trustees, as one of the ablest physicians in America. The published pathological and therapeutic opinions of *such* an official, may not be authoritative with his *pupils*, but may have weight with physicians who, living at a *distance*, cherish respect for the distinguished faculty to which he belongs, and are, therefore, entitled to notice. I shall examine them *seriatim*.

1. Mr. B., it is asserted, died of *Colic*. The stress laid on this word by the professor, would seem to indicate, that he regards colic as a disease not less specific than the small pox or hooping-cough. In his adherence to the antiquated nosology of the school in which he was educated, he seems to be unaware, that the word colic is vaguely applied to a group of symptoms, consisting of intermittent abdominal pain, constipation and swelling and that these symptoms may arise from a great variety of lesions, and be, or not be, accompanied by inflammation. In the case of Mr. B. what produced the colic—that is, the pain, inflation, and costiveness? On the day previous to his attack in the evening, he was in his usual health, and did nothing, suffered nothing, ate nothing, drank nothing, that could have thrown the muscular tunic of his bowels into morbid action. The attack was sudden and violent, and unproduced by any obvious cause.

But the Professor seems to regard an inflated state of the bowels, as characteristic of *colic*, and consequently absent, after death from any other disease! Who has ever made *post mortem* examinations, without meeting with inflation of the bowels, when none of the symptoms of colic existed during life? Who has not witnessed the abdominal distention, which precedes death from the bilious fevers of the West? Who does not know, that acute peritonitis, when it proves fatal, is attended with abdominal distention? As a specimen of this, I will quote the following cases from Cooke's Morgagni:

"A young man, twenty-two years of age, was wounded in his groin by the horn of a cow. He had fever, and by degrees his face and the remainder of the body began to swell, but the swelling was not strictly œdematous. Besides this, nothing of importance resulted till the seventeenth day, when respiration became difficult, and these affections were accompanied with pain, and the sensation of a bolus occupying the fauces. He was also occasionally agitated by tremor, and the tumefaction of the body not only continued but increased. The swelling which occupied the groin extended, and he died about the twenty-second day.

Dissection.—The wound began near the abdominal ring, and extended between the rectus muscle and the tendons of the obliqui; and in this space a considerable quantity of coagulated blood was found. *The intestines were greatly inflated;* there was a copious redundance of serum in the abdominal cavity, and a similar effusion had taken place into the thorax."—*Valsalva, liv.* 2.

"A foreigner received a wound from a thin two-edged sword which penetrated the abdomen obliquely, on the left side, beneath the ribs. The narrowness and obliquity of the wound were such that even a probe could not be introduced. From the time of receiving the wound till the fifth day, (on which he died) he vomited bile, with the ingesta. He was tortured with pains in the belly, and the intestines never acted except by means of stimulating clysters. Towards the close of life blood escaped from the mouth and nostrils.

Dissection.—The intestines were distended with gas, and beneath them a small quantity of blood was extravasated. The sword had reached to the intestinum colon, about four digits below the spleen; but it had injured it only superficially. The weapon then passed onward and wounded the mesentery, which was also found swollen.

Morgagni, liv. 35.

2. The Professor informs us, that during the illness of Mr. B. he believed that he labored under intussusception, or displacement of the bowel, and that his colic was occasioned by that anatomical lesion. If either of these accidents had existed, what was the appropriate treatment? For myself, I answer, large doses of opium, tobacco injections, blood-letting, cupping, the warm bath, copious enemata of tepid water, and finally, mild cathartics. What was the treatment pursued under the theory of that anatomical casualty?—cathartic pills, pills of croton oil, senna tea, a mixture of castor oil, oil of turpentine, and croton oil, incessant, irritating injections, a large blister, and one bleeding not long before the death of the patient, to "*prevent* inflammation." I would ask how these dras-

tics were to restore an invaginated or a displaced bowel? Would they allay spasm, or blunt morbid sensibility, and assuage pain, or obviate inflammation? If invagination or displacement existed, would any harm be done, if inflammation did not supervene? Who ever heard of death from either of those affections, but by the coming on of inflammation; and who would expect to ward off that condition, by the unrelenting and unmixed administration of irritating cathartics?

3. That not less than three feet of inflated ileum was lodged in the pelvis, and that this was a displacement, and the cause of the colic, which occasioned death, is rather a startling enunciation to come from a learned Professor. I shall not inquire how *three feet* of *inflated* bowel could be condensed into the male pelvis! Dr. Richards, who was present at the dissection, supposed that there were 12 or 15 inches. But whether more or less, it is agreed by all, that the bowel was not invaginated, strangulated, or inflamed; how then could its presence in the pelvis, even if that constituted a displacement, occasion the death of the patient? But what shall we think of a Professor, who considers a portion of the small intestines *out of place*, and the cause of death, when he finds it in the *pelvis*—the bladder and rectum both being empty, and contracted to their smallest dimensions? When the organs do not fill up the pelvis, what is there to prevent a part of the small intestines from sinking down into that cavity? Whenever the abdominal parietes are cut through, do not the contents of the abdomen issue forth, and even sometimes hang down to the patient's knees? Has not the late Mr. John Bell written a chapter to show, that the contraction of the abdominal muscles, keeps the bowels at all times so compressed, that even the air of the atmosphere, when the former are wounded, cannot make its way into the cavity of the peritoneum? And does not this compression constantly cooperate with gravity, in causing a portion of the ileum to rest upon the fundus of the bladder and the anterior surface of the rectum? On this point, what say the common systems of Anatomy? Let us make a few quotations from the first works that present themselves:

"The rest of the small intestines having no certain seat or division, is continued by innumerable and uncertain convolutions, not to be described, so as fill out the lower part of the abdomen and the *pelvis.*" —*Haller.*

"The ilium is the continuation of the jejunum, situated in the hypogastrium, and *very often* some part of it in the *pelvis.*"—*Chesselden.*

"When the viscera of the pelvis are empty, a *large* portion of the small intestine is in the *pelvis.*"—*Wistar.*

"The pelvis, as a whole, is a conoidal cavity, having its base upwards and the summit below. Its internal surface forms an irregular floor, on which the *viscera of the abdomen are sustained* in the erect position."—*Horner.*

"The pelvis is a large, irregular, bony cavity, open above and below, which terminates the trunk beneath, and supports or contains part of the *intestines*, &c."—*Cloquet.*

Thus we see, that the Doctor has mistaken the *natural and constant* position of a portion of the small intestines for a *displacement*, and deduced from it a *mortal Colic.*

If these quotations expose the ignorance, in simple descriptive Anatomy, of a cherished professor, and thus cast a stigma on the institution of which he is one of the luminaries, the fault is not mine but his; for he gratuitously thrust himself, and rudely dragged me, before the public; and if the exposure which I have made, should give offence to the state of Ohio, which is fostering him for the benefit of her sons, I shall regret it; but will say, as their friend, that, if what I have written should prove a stimulus to his improvement, and secure better teachings for them in future, I shall have something to solace me under her censure.

4. That an escape of the bile from rupture or ulceration of the gall bladder, is not of necessity, a fatal event, certainly requires proof. To say that there is a case on record, which terminated favorably, is rather a singular assertion. I would ask, if the patient did not die, how it could be known, that his gall bladder was ruptured? No *student* of Medicine could confound the effects of a *wound* of the gall bladder, with those

of its rupture from distention, or the escape of its contents by a perforating ulcer. In the first case, the wound might happen when the cyst contained but little bile, and heal up by the first intention, thus preventing a further flow, even after a few hours, but nature has no such salutary resource, when the parieties are torn or eaten through by ulceration. The bile then continues to flow, and must always prove fatal. Even simple punctured or incised wounds of that organ, are generally followed by death. The instances of recovery are but exceptions to the rule: in cases of rupture and ulceration, there can be no exception to the law of a fatal result.

5. Another gratuitous assertion, is that the rupture in Mr. Brooke's case did not occur till the day before his death. The symptoms appear to me to call for a very different conclusion. The patient was in his usual health, when suddenly seized, without any obvious cause, with acute pain in the *region of the liver*, not the umbilicus, as in colic. This pain, obviously, arose from the first, perhaps a moderate, escape of the bile into the cavity of the peritoneum. He walked about, and in a few hours, the pain was tranferred to the hypogastric region, where the bile would of course gravitate. From this time forward his sufferings were acute, his bowels costive, and his abdomen swollen and tender under external pressure. Thirty-six hours afterwards, when he was rising up in bed, another discharge seems to have taken place. The opening in the neck of the gall bladder, was no doubt enlarged, and the cyst then probably emptied itself. No new symptoms arose, but the old became more intense. Thus the whole history of the case indicates, that all the symptoms came from the progressive escape of bile into the peritoneum, and its irritating action upon that membrane.

6. That the cupping which I attempted, was unjustifiable, because the patient died in six or seven hours afterwards. Suppose he had died in one, what harm would the cupping have done? If he had been *in articulo mortis*, it could not have been felt, and would not, therefore, have increased his sufferings: if his sensibilities were still so acute, that the application of the scarificator gave him pain, a well founded

hope might be cherished, of its doing him good, provided he labored under inflammation. The fact is, that he was not in the jaws of death, nor insensible; but took an active interest in the operation, and complained in the usual degree, not only of the sarification, but of the pressure of the cups. I would put it to the profession to say, whether in peritonial inflammation, when general bleeding is no longer admissible, cupping and leeching, are not our most precious resources; and whether any physician would be justifiable in omitting them, on account of the pain they might give the patient? On this point, I believe there can be no difference of opinion.

7. Lastly, it has been affirmed with great confidence, that in Mr. Brooke's case there was no inflammation. An inquiry into this matter cannot be unprofitable. 1. The authorities cited in the paper which I read before the Medical Society, establish the fact, that the escape of bile into the peritoneum, generally excites inflammation; though, in a less degree, I believe, than an escape of the contents of the stomach and bowels. Now in Mr. Brooke's case, it is admitted, that there *was* an effusion of bile upon that membrane. And a cause of inflammation therefore existed. 2. The gall bladder was so contracted, as to be buried in the liver; and it is not probable, therefore, that it contained much bile at the time when the perforation of its neck occurred. If so, whence came the *quart* of ropy fluid, which the peritoneum contained? Undoubtedly, in a great degree, from the surface of that membrane, indicating an increased secretory action, which carried off the turgescence of the capillary vessels. 3. The surface of the bowels, the liver, and the diaphragm, was covered with a "bilious deposite," which according to Dr. Smith, was thickest on the *under* side of the last organ. But if it had been a deposite, it should have been thinner there, for deposites cannot be made upwards. I have no doubt that the "bilious deposite," was an exudation from the surface of the peritoneum, colored by the bile, and a consequence of inflammatory action. 4. If there was no inflammation, what produced the size on the surface of the blood drawn the night

before the patient's death? 5. What could create such great tenderness of his abdomen, (rendering pressure intolerable,) but inflammation? 6. The absence of a loaded state of the capillaries after death, is not conclusive evidence that inflammation had not existed. On this point many authorities might be cited, but shall content myself with one. Dr. Eberle (*Theory and Practice of Medicine, Vol. II.*) after speaking of the inflammatory nature of Anasarca, observes:—

"Subacute inflammation of this membrane, (the peritoneum,) in whatever way it may be produced, terminates perhaps always in effusion; although in some instances, this may not be so copious as to constitute dropsy. In the majority of fatal cases of ascites, the peritoneum exhibits a highly injected state; and in many instances, the traces of previous inflammation are still more conspicious and unequivocal, its structure being either thickened and otherwise altered, or covered with an infinitude of miliary tubercles. Occasionally, indeed, no marks of pre-existing inflammation whatever are to be seen; but the investigations of modern pathologists have rendered it abundantly manifest, that where no disorganization or structural change has been effected, the mere redness, or injected state of inflamed parts, may and does often disappear in *articulo mortis*, or even *post mortem*."—*p.* 429.

From these facts and considerations, I do not hesitate to express the opinion, that inflammation did take place in this case, but do not believe it was very acute. The temperament of the patient was eminently the *nervous*, and the disease which was awakened in the peritoneum partook largely of the character of irritation. But the question relative to Mr. B. is *not* whether he died from inflammation of the peritoneum, or from irritation of that membrane. The inquiry is this—Did he die from a disease of the peritoneum occasioned by an effusion of bile, or from colic produced by the lodgement of a portion of the small intestines in the pelvis, its natural situation? That it was from the former, seems to me absolutely incontrovertable; and such was the opinion expressed by Drs. Smith and Eberle, immediately after the *post mortem* examination was made.

March 1*st*, 1834.

Art. II.—Nitrate of Strichnine in Paralysis.

A short account of the beneficial effects of the Nitrate of Strichnine in Paralysis, in a letter to the Editor.

Meadville, Pa., Feb. 20th, 1834.

Sir,

I take the liberty of communicating to you, in a brief sketch, of the result of my experience, with the *Nitrate* of Strichnine, in the cure of Paralysis.

This form, or preparation of Strichnine, is to me a new one, as a medicine. The disagreeable symptoms resulting from the use of the Nex Vomica, or Strichnine, has prevented my paying much attention, to any of the other forms.

Dr. Kruse, a German Chemist, now residing in this place, having made a very beautiful preparation of this article, I was induced to try it, and I am happy to say, it more than answered my most sanguine expectations.

My first trial of this medicine, was in the case of a young man, 24 years of age, who had been laboring under severe Scrofula, for about three years; for which he had been under medical advice during a year's residence in Baltimore, and about the same length of time in Pittsburgh, with little or no relief. His case was pronounced incurable; and as a last resort, he was directed to use Swaim's Panacea.

When he had taken about half the contents of a second bottle, Ptyalism was produced, attended with great debility of the whole system. The ulcers became very painful, with coldness and pain in the extremities, which increased until he lost all power of motion. The Panacea was then abandoned.

He remained in this helpless condition about two months, when he applied to me for relief from his severe sufferings, though he had given up all hope of recovery.

His pulse was frequent—small and somewhat wirey—respiration hurried—skin smooth and glossy, but little or no swelling underneath—tongue clean. He had a large ulcer directly

over the sternum, about six inches in the greatest diameter, and four in the least, discharging a bloody sanious matter. Another from the right axilla, opening backward, near the edge of the scapula, which also presented the same unhealthy appearance. The left axillary gland produced a tumor about four or five inches in diameter, presenting a hard, uneven surface, very painful.

My first attention was directed to the scrofulous disease, which entirely disappeared in less than six weeks. (The course I have pursued in the treatment of scrofula, for two years past, I intend to present to you at another time.) Unfortunately, however, he remained as helpless as ever. After making several trials of the usual remedies, which proved fruitless, (as I believe they too often do, in cases of long standing,) I commenced administering the Nitrate of Strichnine, in form of solution. (Nit. Strich. i. gr. Dil. Alcoh. ℥ij mix.) Immediately after taking one drachm of this solution, he felt a glow in every part of his system, which continued about three hours, when his former disagreeable sensations began to return. The same quantity was again administered, with the same good effect, and repeated every third hour, increasing one drachm of the solution daily. On the fourth day he was able to feed himself; and at the end of four weeks, he walked across his room with no other assistance than his cane—entirely free from pain—all the functions of the system performing their offices in a healthy manner. In a case of Paraplegia, I was equally successful.

So far as my experience will permit me to judge, I consider this form of Strichnine, as decidedly preferable to any other; as being mild in its operation, and much more safe than either the Nex Vomica or Strichinine. It produces no irritation of the stomach, and instead of exciting spasms, it entirely allays the spasmodic twitchings, often so distressing to paralytic patients.

It was not my intention to give a labored essay on this subject, but merely to express, in a brief manner, the result of my practice in the use of this medicine, hoping it may be a

means of aiding some of my professional brethren, in giving relief to suffering humanity.

I will merely add, that much care is necessary in the preparation of this article, the crystals should form a white, pearly needles—not a brown dust as we often find it in the shops.

Should you find any thing in the foregoing statement worthy of notice, you are at liberty to make such use of it as you think proper.

Yours respectfully,

EDWARD ELLIS, M. D.

DR. DRAKE.

ART. III.—CUPPING.

Remarks on the Operation of Cupping. By W. A. GILLESPIE, M. D. *of Ellisville, Va.*

THE use of Cupping instruments has been known from time immemorial. They are mentioned by Hippocrates and Celsus, as well as other ancent writers. No member of the profession, at this day, can be ignorant of their use and importance; but I fear there are some who, for want of proper instruments, and manual skill in the use of them, too often omit their application, and rely on other means, in no wise as clearly indicated. In the present progressive and rapid march of medical science and improvement, morbid anatomy has revealed many of the once hidden mysteries of disease; and hence a more correct pathology and diagnosis have assigned a 'local habitation and a name' to maladies formerly considered general, or rather incomprehensible. It is to this we are to attribute the increasing importance of local blood-letting as a therapeutic agent. It is not my intention or design, in this

communication, to advocate or support the doctrines of any particular systems of disease; or even to give an outline of the use of cupping as a curative means; which last I leave for those better qualified, hoping your readers may be favored, from other sources, by an essay on that subject: but it is to make a few observations on the instruments, and manual operation of cupping. Various instruments formed out of different materials, have been used at successive periods. Cucurbitulæ, or small gourds, were used at remote eras, and are still used by some; but more recently, cupping glasses have nearly superseded them. The scarcity of leeches, owing to the little attention paid to procuring, preserving, and breeding them, renders cupping still more desirable as a common means of local blood-letting, especially in country practice; the cities being supplied with leechers and cuppers by profession. The common cases of cupping instruments, now used in Virginia, are sold at very high prices, and from their construction, are liable to serious objections, particularly in country practice, from the inconvenience of carrying them about, and the difficulty and attention required in keeping them in good order, &c. A great superiority is claimed for them in consequence of the exhausting syringe adapted to the glasses; but whoever makes the experiment, will find it inferior in actual practice to the old method of exhaustion by the torch. The common scarificator, too, is objectionable, on account of the great number of lancets which it contains, and which, by so great a number of incisions, were supposed to promote a freer discharge of blood, which is by no means the case. I have repeatedly observed, that more blood can be drawn from the incisions of five or six lancets, than from a larger number; and I have frequently seen the blood flow as freely from a single incision made by a scalpel or spring lancet, as from fifteen or sixteen lancets in a common scarificator. Two or three incisions made by a sharp scalpel, razor, or lancet, across a fold of the cutis vera (held or pinched up between the finger and thumb, or by an assistant) deep enough, in common cases, to go through it, will answer as well as the best scarificator, and much better than a dull or rusty one. If the scari-

ficator be used, great care should be constantly taken to have the lancets very smooth, sharp, and free from rust; otherwise disappointment will ensue on applying the glasses, with the expectation of obtaining a free flow of blood. With respect to exhaustion of the glasses by the torch, which is preferable to the syringe, a very convenient and effectual method, is to take small pieces of what is called in the Southern States, *fat pine*, or small slips of any kind of porous wood steeped in spirits of turpentine, and dipped in flour of sulphur and dried, varying in length to suit the depth of the glasses. The end of one of these may be lighted with a candle, and the other end being held between the thumb and fore finger of the left hand, should be placed on the skin about the centre of the incisions, perpendicularly to the skin, or in the intended direction of the glass, which should be applied with the other hand without loss of time, immediately over this little torch, with one edge of the glass resting on the skin until the air is nearly exhausted, when it should be entirely applied without removing the torch, which will immediately cease to burn. The fire cannot touch or burn the patient, in consequence of the torches being longer than the diameter of the glass, on the side of which it will always rest until wholly extinguished. But as glasses are frequently broken in carrying them from place to place, an excellent substitute can be made of a small cow's horn, *Corniculum*, which may be scraped and polished until perfectly diaphanous, or transparent. Several may be used, of different capacities, from 2 to 6 oz. each. The torch may be used in the same manner with these, and they answer much better than glasses, which draw in too much of the surrounding skin, and thus, by compression of the vessels, retard the bleeding. The torches may be prepared before-hand, and conveniently carried in one of the horns or glasses, together with a piece of sponge to clean the incisions. There is another way of exhausting the horn, which is practically much better than the torch, or exhausting syringe, which last may be applied to it as well as to glasses. As it is not prevalent, I believe to any extent, in the profession, and as it may be thought by some to be void of cleanliness or decency, I shall

here ask the indulgence of my more fastidious brethren, and offer as an apology, that no man ought to practice physic, who is not prepared to undergo any reproach or contumely, not implicating his principles of honor or integrity, when his proceeding is designed for the good of his patient. I allude to exhausting the air by suction with the operator's mouth. A hole is to be made through the point of the cupping horn, and stopped with common bees-wax, which, being nearly of the shape of a vial cork, is to be perforated by a common pin into the cavity of the horn, which, thus prepared, is to be applied as soon as possible over the scarifications. The air is now to be exhausted by suction, and at the same time the teeth are to be clenched on the wax, so as to stop the perforation of the pin and prevent the ingress of the air. A little practice will enable the operator to regulate at his pleasure, the degree of suction required. I repeat it, that it is particularly important that the scarifications should be made with a sharp, clean instrument, and that the horn or glass should always be applied without loss of time after making them, to prevent coagulation of the blood in the incisions, which will prevent its running. A metal stop-cock can be applied to the horn, to answer the purpose of the wax and pin; or the exhausting syringe may be adapted to it as well as to the glasses; or a thin piece of gum elastic may be confined over the perforation of the horn, which will admit of exhaustion by suction with the mouth. By this the fire or torch, one cause of dread or alarm to the patient, is removed. A similar instrument, cornicula, is described by Hildanus, cent. I. obs. 80. "It is made of horn, almost in the form of a cupping glass, except at the more slender extremity there is a perforation. The wide end is laid upon emaciated parts, and a person applying his mouth to the perforation at the small extremity, by suction draws out the air. In consequence of this, the part covered rises in the hollow of the instrument, and by this means the nutritious juices are thought to be invited to the emaciated part." Hildanus relates a cure performed by this means, and gives a figure of the instrument. Tulpius, lib. III. obs. 49, gives another instance of a cure performed by this means.

The reader will readily perceive that I have been very minute, if not tedious, in some of the foregoing descriptions, but a desire to be clearly understood by all, and to profit some, must be my apology for the same. The plan of cupping with the horn here recommended, if it has not the recommendation of neatness and show, (which in most cases is not to be disregarded) has the more substantial one of superior utility. It simplifies the operation, and adapts it peculiarly to country practice, where, as a general thing, it is most needed, as the towns (and not the country) are supplied with cuppers and leechers by profession. It obviates the necessity of procuring expensive instruments for cupping, and the inconvenience of carrying them about. It excites less dread in the mind of the patient than a formidable display of numerous and complicated instruments. This last is a matter of some importance, as the cases requiring cupping are often serious, and a strong impression of fear and dread made on the mind, may give, perhaps, a fatal turn of the scale against the languishing patient. From the simplicity and little show of the instruments and plan here recommended, no such impression is likely to be made on the patient's mind.

Any person of common dexterity, may adapt horns to the use of cupping, and a short scalpel, razor, or lancet, may be used to make the incisions. And however simple this plan may appear, I speak confidently and from several years experience, when I say, that a certain amount of blood can be sooner and more easily procured in this way, than with the most complicated and expensive instruments. I have had experience with the different instruments and modes of cupping, and am clearly of opinion that with all who prefer utility and convenience, the horn must supersede them, with some it has already done so within my acquaintance, and the conviction is daily getting stronger by experience, even with the most clamorous in favor of the syringe, glasses, &c. Some have abandoned them, and now use the horn only. In making these statements I am uninfluenced by any thing but a desire to find and communicate truth to my brethren of the profession, who will, if they make the experiment, see for themselves the

grounds for them. They are the results, not only of my own, but of the experience of others in this part of the country, and I hesitate not, cordially to recommend the practice to others.

January, 1834.

Art. IV.—Operations for the Removal of Urinary Calculi.

Some observations on the various means of relief that have been proposed, or adopted in cases of Urinary Calculi. Read before the Cincinnati Medical Society, by A. Hermange, M. D.

Of all the physical ailments, incident to our nature, no one has more occupied the zealous and indefatigable attention of the profession than stone in the bladder. The circumstances attending their presence, particularly in the male bladder, are indeed of so painful, and distressing, and formidable a character to the patient himself, and when considered in reference to remedial operations, are so trying and perplexing to the professional attendant, that it is not astonishing that medical science and art, always devoted to the cause of human suffering, should be exhausted in devising ways and means of relief.

Every method must, from the nature of the case, answer one or the other of two great indications,—either to destroy the calculus, or lessen its irritation, whilst in the bladder;—or to cut a passage for its extraction. We speak, of course, of all cases, where the stone, from its dimensions, cannot escape through the natural, or dilated passage of the urethra.

Accordingly, in medical history, we find various lithontriptics recommended to answer the first, or more merciful indication, which, it was hoped, might dissolve urinary calculi; or at least check or stop their further increase;—various

menstrua which, by being injected into the bladder, might effecttheir decomposition and solution, or mere solution. But it has unfortunately been found by experience, that although concentrated chemical agents in a short time, or largely diluted ones in a longer period, can effect those objects in the chemical laboratory; still, when similar agents are made to exert their influence through the general circulation, they will no doubt be modified, in their progress to the bladder, by the chemical influence of the fluids of the system itself; and the only good we can hope for, (and that even is problematical,) is to arrest their further progress, by altering the chemical character of the animal fluids. In a concentrated form, the remedy would in a short time, prove itself worse than the disease. We are therefore compelled to use such articles (whether introduced into the stomach, or injected into the bladder) in a highly diluted state, and then, simply for the purpose of palliating the symptoms, and lessening irritation. The health and vigor of the constitution would give way, infallibly, under the continued treatment necessary to make much impression upon the stone. We learn from the writings of Dr. Whytt, that the minister Walpole was cured by the remedy of Mrs. Stephens; but his death, at a later period, was attributed to the effects produced on his system, by the too great quantity of soapy, or alkaline matter required for the destruction of the calculus. Dr. Hartley too, the great advocate of Mrs. Stephens' nostrum, is said to have died of the disease, after ineffectually taking more than two hundred pounds of soap, the main ingredient in her medicine. All that we can now say of this case, is, that it is wonderful, how his constitution held out so long under such treatment, and that supposing his system to have been affected with a phosphatic diathesis, we know not how far the exhibition of acescent lithontriptics, to an equal extent, would have availed.

At that period the public attention was all alive to the important subject: but little, however, was done. Subsequently, as chemical science was developed, Scheele, by the discovery of the uric acid in the most common varieties of stone, di-

rected the attention of other distinguished men to the subject. And the luminous researches of Wallaston, Fourcroy, Vauquelin, Pearson, Brande, Marcet, Prout, Henry, Thenard, &c., certainly inform us of the true chemical nature of these urinary concretions; but they likewise teach us caution, with regard to the proper methods of correcting calculous diathesis. In the urine, according to Mr. Brande, uric acid and the earthy phosphates are always present, and these last are mainly held in solution by their excess of acid. If then, as Mr. Brande has suggested, a concretion of uric acid were present in the bladder, and we were to administer some alkaline solution, with the view of forming a soluble urate, the exhibition of it, beyond this point, might likewise neutralize the excess of phosphoric acid, and give rise to a precipitation of the earthy phosphates, which might go to form an additional crust around the calculus, before this last could be reached by the dissolving alkali. On the other hand the exhibition of acids, for the purpose of arresting any further precipitation of the earthy phosphates, and of removing those already formed, has, after some time, occasioned a deposition of uric acid. We might indeed, by the exhibition of alkaline remedies, carried to a certain extent, lessen the irritation produced by the mulberry calculus, chiefly composed of the oxalate of ammonia, by favoring a precipitation on its rough, unequal surface, of a smooth layer of phosphate of ammonia and magnesia; but this is only correcting one evil, by the substitution of another, in the increase of volume, which must render the ultimate aid of surgery more precarious.

Chemistry then indeed has enlightened the practitioner; but it has only done so by exposing, in a clearer point of view, the difficulties of the subject. It teaches us, (as has been already remarked,) that nothing can be hoped for, in the way of dissolving large calculi at least; and only urges prudence in our attempts to correct depraved habits of body. This is, however, an important point gained; for although chemistry has left such sufferers to nearly all the horrors of their situation, still, they are not subject to have those ills ag-

gravated by zealous and humanely intended; but, at the same time, blind and ignorant interference.

Having a full view of the insufficiency of acid, or alkaline menstrua to dissolve calculi, as proposed by Fourcroy, Vauquelin and others, and of their tendency, when persevered in, to provoke inflammation, as well as, when unscientifically administered, of sometimes enlarging the stone; M. Jules Cloquet, with a praiseworthy enthusiasm, but guided more by feelings of humanity, than by a firm conviction of its eventual utility, offered to substitute pure, or distilled water, either as a menstruum, or to act by attrition. For this purpose, he had an apparatus constructed at the hospital of St. Louis in Paris, which failed, to be sure, in accomplishing the desired object; but which has been, for some years back, employed advantageously in other affections of the bladder. It consisted of a double sound introduced into the urethra, one of the tubes of which was connected, by the intervention of a long conduit of gum elastic, with a reservoir somewhat elevated:—by this means a constant supply of water, at a temperature little rising of 100 degrees of Fahrenheit, was kept up to the bladder. A similar conduit, connected with the other tube of the sound, carried off the water, after it had served its purpose. No doubt can be entertained, that a current of water, kept up a sufficiently long time, would at length wear away a calculus even of the hardest texture, as even rocks, in the beds of rivers, show its slow powers of attrition, and even of solution:—but however bland and unirritating the fluid might be, and how long soever the bladder itself might bear such an affusion, no one, in this way, could in sober earnest, expect success. We have mentioned, however, the plan of M. Cloquet, for the double purpose, of referring to the apparatus at the hospital of St. Louis, as a standing monument of the inefficacy of all menstrua to dissolve voluminous calculi, at least whilst lying loose in the bladder, and of adverting to the benefit to be derived by similar affusion, to a moderate degree, in certain chronic irritations and catarrhal affections of that viscus.

As in all cases of vital importance to mankind, the human mind is, *at all times*, active in unfolding the resources of nature, it very often happens, that the discoveries of the passing moment belong really to times past; and that kindred intellects, of different periods, by striking upon the same vein of thought, arrive at the same conclusions; without any knowledge on the part of the last investigator of what was done before. In this way, frequently, the learned lore of bygone ages, which from fortuitous circumstances, had either been lost, or buried under the accumulated mass of intellectual rubbish, is reclaimed; and although not, in this case, with the *intended* beneficial result to suffering humanity, still, in many cases, (and we will notice some in the sequel of these observations,) bringing honor and fame to (we will call them) the discoverers themselves, and momentous advantage to mankind. A mode of treatment, precisely similar to that of Jules Cloquet, was published in London in the year 1740 by Stephen Hales; and a few years afterwards the work was translated into French; but of this J. Cloquet certainly knew nothing. The plan has had its first sleep, and as a remedy for calculous affections, may now sleep forever.

The first meditations of Dr. Civiale, in the years 1817 and 1818, on the subject of saving calculous patients from the surgical knife, resulted in devising a plan, by which he hoped to be able to dissolve calculi, enclosed in a bag, introduced into the bladder. A straight sound was first to be introduced into the urethra, and through this, another tube, with a double conduit, was to be passed. The inner extremity of this latter tube was armed with two branches, to which the open end of the bag was attached. In this way he could, at pleasure, open and shut the bag, somewhat after the manner of the clasp of a purse. One of the conduits of the inner tube communicated with the bag, and through this the lithontriptic liquid was to be passed; the other communicated with the interior of the bladder, by which the urine could be voided; or if occasion required it, some liquid could be thrown into that viscus, capable of neutralizing the action of the lithontriptic, in case any of it should escape from the bag internally. But

it must be evident, that the almost positive certainty of this occurring, (independently of the permeable character of the bag itself,) constituted the principal vice of his first method. In order to determine the nature of the solvent to be used, he proposed to employ another instrument previously, by which drilling out portions of the calculus, he could submit the detritus to chemical analysis. This accesary, and purely mechanical method of destroying stone, soon, however, took up his whole attention, as the only practicable one in his estimation, and the attempt at solution was abandoned.

A few words will, in the subsequent part of these observations, be devoted to the "Procede Civiale," which, although not adequate to all cases, has proved, and will always prove extensively useful to the unfortunate portion of mankind concerned, has sheda brilliant lustre upon French surgery, and gained imperishable fame for its author. For obvious reasons, the original idea of Civiale failed, and drew upon it and all similar attempts, in the report to the Royal Academy of Sciences, the character of "une espece de chimere." It was given up to pursue another, that promised immediate success, and without the least exertion to correct, if possible, radical defects—defects in the mechanical conception, and in the preparation of materials or making the bags.

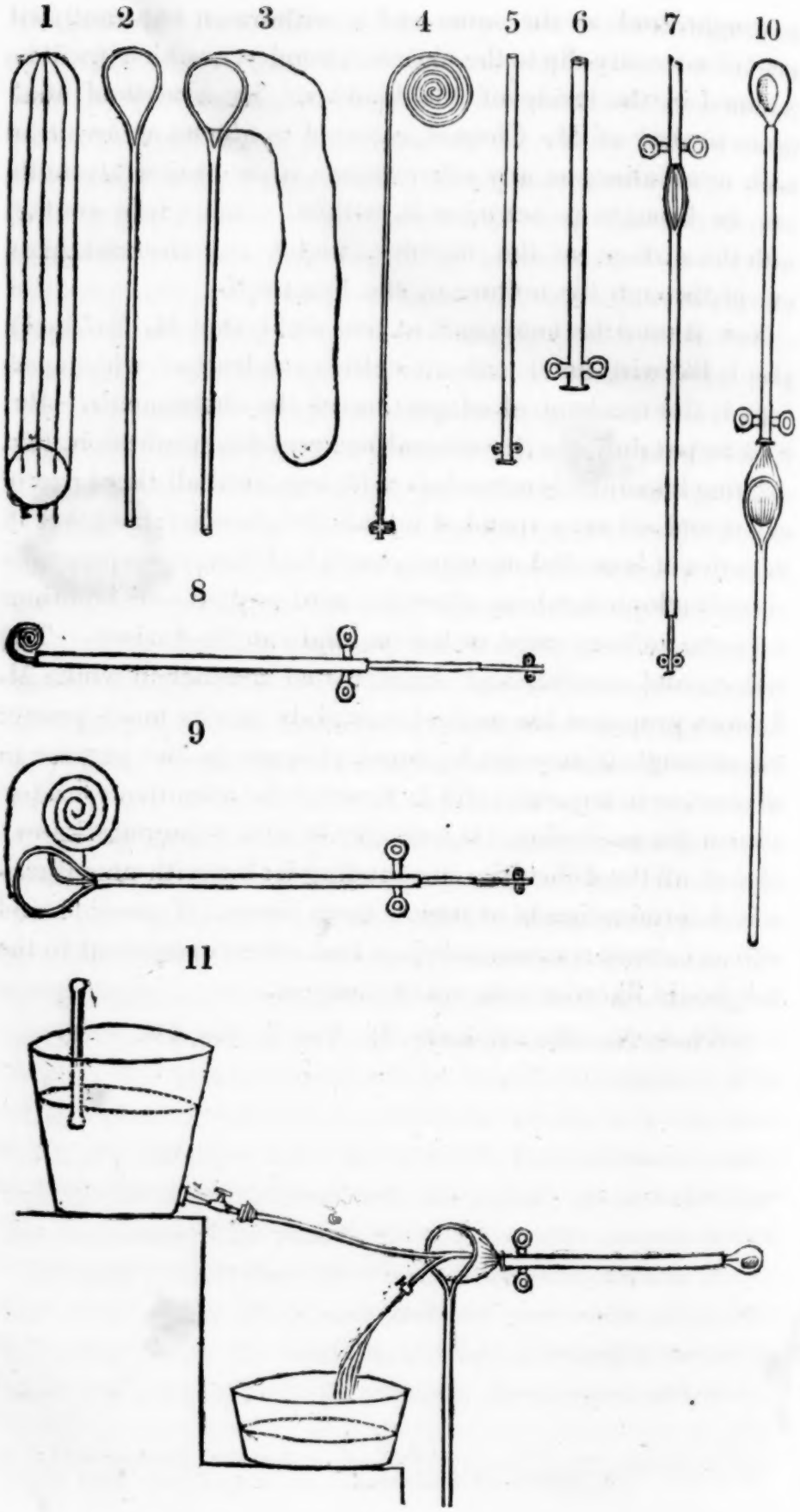
Whilst the writer of these observations was in Paris, a modification of Civiale's original plan was, however, proposed by M. Robinet, which had at least the merit of considerable ingenuity, whatever may be the final judgement upon it by the profession. It was condemned by Dr. Civiale himself, at that time, as having come to no practical result; but this was at least, a premature judgement, as M. Robinet was then only making his investigations, and had merely given to the world the rudiments of his plan.*

It was a contrivance, by which a *long* bag open at one end, and closed at the other, and made of some suitable material, is conveyed into the bladder, through a straight sound, previously introduced into the urethra. In the open end the stone

*The ensuing account of M. Robinet's plan has been principally taken from the Repertoire Generale d'Anatomie, &c.

is caught, and as the same end is withdrawn externally, it must of necessity slip to the closed extremity, in which it will be retained in the inside of the bladder. By a method analogous to that of M. Cloquet, referred to above, a current of acid, or alkaline, or any other liquor, of a proper strength, can be brought to act upon it, without coming into contact with the surface of the bladder, under any circumstances, except through the texture of the bag itself.

Now it must be confessed, at first sight, that M. Robinet's plan is likewise beset with difficulties, the least of which, perhaps, is the mechanical adaptation of the instruments. But let it be put down to the eternal honor of the profession, that suffering humanity is never left to its fate, until all the stores of nature and art are expended in vain for its relief; and that in the series of remedial measures, harsh and dangerous processes are only adopted, when, after the most patient and laborious research, nothing safer, or less painful can be devised. The modest, and candid, and disinterested manner in which M. Robinet proposed his method, certainly merits much praise: and although it may not be found adequate to the purpose in all cases, or in any case, still it deserves the attentive consideration of the profession. It was offered with a thorough knowledge of all the difficulties surrounding it; but with an enthusiastic determination to overcome them himself, if possible, and with an earnest recommendation, that others competent to the task should likewise take up the subject.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The mechanical part of his plan may be stated, in a few words, in reference to the figures.

Fig. 1. Is a cylindrical, membranous bag, prepared from the cœcum of a sheep; naturally closed at one of its extremities. Its length should be about one foot. Its diameter variable, according to circumstances.

Fig. 2. A cylindric tube, to one end of which, is attached a highly elastic, circular spring, to which the open end of the bag is to be fixed. The circular spring must of course have a larger diameter than that of the calculus.

Fig. 3. Represents the bag fixed to the spring.

Fig. 4. Stilet, one part of which, possessing considerable elasticity, is so made that, when left to itself, it will bend round, like the spiral spring of a watch. The spiral portion of it should be at least ten inches long.

Fig. 5. Straight sound, one foot long, which can allow of the introduction through it of the stilet.

Fig. 6. Canula to be introduced into the urethra and which will permit the ingress and egress of the rest of the apparatus. It should have a diameter of about three lines and a half, and nine inches in length.

Fig. 7. Shows the elliptic form which the circular spring (Fig. 2. to which the open end of the bag is attached) assumes, in passing through the canula, Fig. 6.

Fig. 8. Represents the closed end of the bag, pushed by the end of the spiral spring of the stilet, and both escaping to the interior of the bladder. As they enter, the spiral spring, being left free, will wind itself up, together with the bag.

Fig. 9. Represents the bag and spiral spring, escaped entirely into the bladder. The open end of the bag is likewise seen distended by the circular spring of Fig. 2.

Fig. 10. Disposition of the apparatus, when the calculus has been introduced into the bag, and the open end of this drawn out externally.

Fig. 11. Represents the apparatus in action. A sound, with a double conduit, is seen to permit the free ingress and egress of the dissolving menstruum into that portion of the

bag containing the stone. A thermometer regulates the temperature of the lithontriptic solvent.

The different parts of the apparatus are to be used thus;—After moistening the membranous bag, and oiling it, as well as all the other instruments, the stilet (Fig. 4.) is to be passed into the straight sound, (Fig. 5.) by straightening out the spiral portion of it. Both are then to be passed into the bag, in such a manner, that the end of the sound and stilet will touch the closed extremity of the bag. The whole is then to be pushed into the canula, (Fig. 6.) which must be previously introduced into the urethra. When the extremity of the bag has reached the interior of the bladder, the straight sound, which has hitherto controlled the elasticity of the stilet, is to be withdrawn. The elasticity of the circular spring, (of Figs. 2 and 3.) is now to be so far overcome, that it will enter the canula, and by pushing it to the interior of the bladder, at the same time that the stilet is advanced inwards, the whole of the bag will be made to enter. As the bag enters it will be wound upon the spiral spring; and when the whole of it has been introduced, its open end will be dilated by the circular spring, (see Fig. 9.) The operator will then endeavor to catch the stone in the bag, and when this is accomplished, the circular spring and stilet are to be withdrawn together. When the former has escaped out of the canula externally, the latter is to be drawn out entirely, (see Fig. 10.) The sound with the double conduit is now to be introduced, and the process carried on, (as represented in Fig. 11.) as long as the patient can support the presence of the apparatus. In this last part of the operation, it would, without doubt, be preferable to remove the canula, which, from its size and form, would soon be found too irritating to the patient, and to leave only the bag and double sound in the urethra. The double sound should also have a slight curvature, and be as flexible as possible, for obvious reasons. At the close of each operation, the bag should be washed out with simple water previous to the withdrawal of the instruments. Perhaps the only practicable way of withdrawing the bag, after the first operations, if the stone remained of any size, would be to rupture its

closed extremity; and in the subsequent operations fresh ones could be employed.

But the great problem yet to be solved, is—of what material are the bags to be made? and how prepared? so that the four great requisites may be attained of strength, thinness, pliability, and ability to resist the chemical action of the various solvents, necessary in different cases? To determine these matters, M. Robinet has already made a great number of trials. He had prepared, under his direction, single, double, triple, and quadruple membranes, and in close juxtaposition, so as to have bags of various strength. His first experiments had a reference simply to the physical resistance of the membranes, and he arrived at the conclusion, that a double one had all the strength requisite. In the next place, he occupied himself in observing the action of various menstrua upon the unvarnished membranes, and introduced into single, double, and triple bags the following liquids;—

1st. A solution of caustic potash, in the proportion of sixteen parts of water to one of alkali.

2nd. Hydrate of lime diffused in water, (lait de chaux.)

3d. Sirupy phosphoric acid, diluted with twelve times its quantity of water.

4th. Common vinegar.

5th. Muriatic acid, diluted with six times its weight of water.

Bags filled with these several liquids were suspended in the open air. At the expiration of twenty-four hours none of them were found to be torn; but they had suffered part of the liquid to ooze through. The single bags contained but a small quantity, the double ones a considerable portion, and the triple ones had scarcely lost any. After some days exposure, in this way, the most of them were rent; but this occurred precisely in that part of the bag, immediately above the liquid, and on which, of course, the corroding liquor acted in a more concentrated degree, by the evaporation of its water. It results, from these trials, that the membranes, of themselves, offer considerable resistance to the action of those solvents, since it was only by simple sweating through, that they

escaped during the first twenty-four hours. It may easily be conceived, however, that a bag thus filled, and surrounded by the urine in the bladder, or by any other bland fluid, artificially introduced there, would be placed under much more favorable circumstances, and so far from the caustic liquor oozing into the bladder, and mixing with the urine, we might expect the opposite effect to take place, viz. the urine, by the pressure of the bladder upon it, to ooze into the bag, where there can be no pressure. In this way too, the urine occupying the pores of the membrane, would prevent this last from being so effectually corroded by the caustic liquor.

M. Robinet's next great object was to determine what kind of varnish would render the membranous bags most impervious, and for this end, he likewise instituted many experiments. The character of the solvent must, of course, determine the nature of the varnish. It appeared to him that, that usually employed in varnishing taffeta silk answers the purpose very well for acid menstrua. Accordingly, after having given some of the bags a coat of linseed oil, rendered drying by litharge, he suffered them to dry properly, and submitted them to several trials. Even tolerably concentrated acids had no action whatever upon them; but a solution of potash, in a very short time, took off the coat of varnish.

Gum Caoutchouc likewise attracted the attention of M. Robinet; but with a full view of all the difficulties attending its employment. Its application was nevertheless attempted by several processes. He successively dissolved the gum elastic in sulphuric ether, in the volatile oil of turpentine, in linseed oil, and lastly in a peculiar oil sent to him by M. Payen, which has the property of dissolving it even when cold. By means of these different preparations, he succeeded in rendering the bags incapable of being acted upon by any of the solvents; but they all had one weighty inconvenience—it was found impossible to dry them so that they would not stick when the surfaces came into contact. M. Robinet, however, with a rare and truly philanthropic zeal to attain the desired object, was not to be deterred—and after diversifying the mixtures of the above solutions, he had recourse to alco-

hol, which has, as it is known, the property of precipitating caoutchouc from its solution. He hoped, by the agency of this fluid, to dissolve the portions of volatile oil, still in connexion with the varnish, and thereby to leave the caoutchouc pure at the surface of the membrane. But success still eluded his pursuit. He expected after some time to continue his essays, with the juice of the tree which produces the gum elastic; when, it is to be hoped, the proper result may be obtained. We have not had an opportunity of learning, within these few years back, what has been further done on this important subject.

He proposed to himself, as a last subject of inquiry, to ascertain, 1st. What are the best menstrua for the various species of calculi? 2dly. How much time must be employed in dissolving a calculus of a given volume and weight? 3dly. Whether there is any difference, and in what it consists, with regard to the solubility of recent and dried calculi? 4thly. What advantage can result from the employment of the different solvents at different degrees of temperature. 5thly. What benefit might result from the substitution of a current of the dissolving liquor, in the place of allowing the calculus to remain in a certain quantity of it for some time? Difficulties would, of course, arise in the cases of calculi having alternating layers of different substances, requiring different solvents; as well as in ascertaining with positive certainty, the exact chemical character of a calculus, whilst in the bladder. The analysis of the urine, and the general diathesis, might throw much light upon each case in this respect; but it is to be feared they would not afford that precision of information so necessary in such a method of treatment. With regard to the simple application of the instruments to the living bladder, (should every other indispensable requisite be successfully responded to) there would scarcely appear to be more difficulty than in the case of Dr. Civiale's method of lithotrite. In concluding the notice of M. Robinet's plan, might not it, (if eventually found practicable) and the "operation Civiale," assist each other occasionally? In cases, for example, of extremely hard and voluminous calculi, might not the latter, by a first essay, expose enough of their detritus to give information

of their chemical character? When the former might be brought in to complete the noble work of destruction; and thus, indeed, accomplish the first conception of Dr. Civiale. The anticipation of many an undoubted, and indeed, familiar fact, of the present day, would have equally been deemed, twenty years ago, "une espece de chimere."

Lest any thing should be left undone in such a cause—lest a *single stone* should be left untouched in the glorious field of investigation, we might say, of destruction, MM. Prevost and Dumas proposed, but a few years ago, to effect the destruction of calculi, by placing them in the circuit between the poles of a galvanic battery. The two conducting wires were to be insulated, in their passage to the inside of the bladder, by being introduced through a gum elastic sound with a double conduit. The agency of such a principle, we all know to be powerful enough for almost any purpose, (certainly for the purpose of disintegrating, as well as breaking up the chemical character of calculi in the laboratory); but to the enlightened practitioner of medicine and surgery, what a host of formidable, insurmountable obstacles array themselves at once against its application in the living human bladder! The names of such men, engaged in even such a hopeless pursuit as this, merit not the less, the meed of praise; but when the most learned and dexterous experimenters are thus driven to extremity, in their laudable and merciful exertions, a correct estimate may be formed of the painful perplexity of the case. The old saying, 'there is nothing new under the sun,' happened to be true of this method likewise—for some ten years before MM. Prevost and Dumas gave their views to the scientific world, Dr. Gruithuisen of Bavaria, had not only proposed a similar mode, but had publicly insisted, that he could succeed with 600 galvanic disks; and Mauduit, of Varenne, had, still previous to that, advised a trial of the agency of electricity.

Previous to our few remarks on the method of lithotrite, it may not be amiss to take a cursory glance at the practice of the Egyptians, of mechanically dilating the urethra, during the time of Prosper Alpin, at the close of the 16th century. He himself was a witness of the practice he describes. He

relates the case of a military chieftain, from whom several stones were, in this way, extracted by a distinguished Arab. He made use of a canula of wood, in length about 8 diameters of the finger, and of the thickness, or diameter of the thumb—With this, he blew with force into the canal of the urethra—to prevent the air from penetrating into the bladder, he took care to press strongly, with his other hand, the other extremity of the urethra against the arch of the pubis. After thus forcing air into the urethra, he kept it there by closing the external orifice of the canula—then introducing the finger into the anus, he gradually directed the stone towards the orifice of the neck of the bladder, and from thence into the canal of the urethra. When it was thus brought near the meatus urinarius, he withdrew the canula, with violence, from the urethra, and in this way procured the discharge of a stone the size of the kernel of an olive. Prosper Alpin afterwards assisted at two other operations, practiced in the same manner, by the same man—in one case, that of a child, he withdrew eight calculi; from the other, an adult, he extracted a stone of the volume of a large olive.

He mentions likewise another, and no doubt a better, process, by another Arab practitioner, and which consisted in introducing, successively, into the urethra, a series of flexible and dilatable canulæ, and dilating them by forcing the air into them. We suppose that all, except the last canula, had their inner extremity closed to prevent the entrance of the air into the bladder. The canulæ were of three or four different diameters, and when the last and largest was introduced, the operator passed his finger into the rectum, and endeavored to bring the calculus into the inner opening of the canula; its further entrance into the canula was to be promoted by suction at the external opening. In one case of this kind the stone was broken.

Now we may remark of these processes, that they were sufficiently rude, but if the cases be true, as stated, it might lead the mind to inquire into what extent the urethra might be dilated by more skilful means, were they *slowly* and judiciously employed, for the evacuation of moderately sized

calculi, and for preparing the urethra for the introduction of the straight sound in the 'procede Civiale.'

The last method of answering the first great indication, that we will notice, is the "procede Civiale." Within the few years back so much has been said, and written, and done, on this interesting development of modern surgery—it is now so completely the property of the surgical world, that but little need be said of it, and that in very general terms, in addressing ourselves to the learning and intelligence of the profession.

Dr. Civiale, between the years 1817 and 1824, (the period of the first conception and ultimate perfection of his processes) was one, (of the many distinguished men in the records of history) who, with talents and devoted zeal, had to contend against the insufficiency of fortune, and the prejudices always springing up in opposition to every thing of a novel character. In the year 1818 he called upon the French government for aid in prosecuting his inquiries, and his memorial was referred to the Royal Academy of Sciences, as the body most capable of determining the merits of the case. But the committee appointed by that Academy, refused to report at that time, and of course aid was denied. The same committee reported favorably in 1824, when the question had been settled by extraordinary individual enterprise, and when, of course, aid was no longer wanting. We mention these facts as illustrative of the honorable disposition of the French government to patronise science, as well as to show the occasional strange fatuity that sometimes comes upon even learned societies, with regard to the dearest *interests* of the sacred cause in which they are engaged. Arriving at the fruition of all his anxious expectations, Civiale now consoled himself by the thought of having done well for his species, and by indulging, in delightful anticipation, the award of posterity. But in examining closely the chaplet of his fame, he discovered the loveliest flower missing, the merit of originality. Gruithuisen, whom we have mentioned before, had, in the year 1813, published a rude and undigested, *but similar* plan. It was, however, merely proposed, and almost forgotten, and but for the genius and enthusiasm of Civiale, might have been forever swallowed up in the

fathomless gulph of the past eternity. To claim the honor of discovery for Gruithuisen, would be about on a par with awarding to the Spaniard, Blasco de Garay, the reputation of determining, at Barcelona, 290 years ago, the practicability of propelling vessels by steam; or in still later times, with less justice, to Savery, and to Hulls: shades of our renowned countrymen! spirits of Evans and of Fulton! what had been the present condition of this Western habitation of civilization and learning, had not you become the virtual, the practical discoverers of steam navigation; at the commencement, even at the risk of appearing as fools for science's sake. We will not advert to individual opposition to the 'procede Civiale,' although that was not sparingly dealt out in France and elsewhere.

Century after century had rolled on, from the first dawn of surgical knowledge, in pursuit of a safe method of destroying stone whilst in the bladder. The various modes had their day of vogue, and were successively laid aside, and only recorded in the annals of medicine, by way of caution, and of sparing useless labor to future investigators. To hope for success, by the application of any principle within the present scope of man's knowledge, seemed almost to be hoping without hope. It was reserved for our own time to solve the great problem, and to show that it can, at least in many cases, be effected, and that too, according to the simplest principles of mechanical philosophy.

It is not necessary, as has been already remarked, to enter here into a description of the instruments, or of the mode of employing them. Suffice it to say, that their ingenuity and practicability have been extensively tested by experiment, and that, *so far*, the 'operation Civiale' is the only one that does answer effectually, in many, perhaps most cases, the first merciful indication. We do not know the number of cases that have occurred in Dr. Civiale's practice of lithotrite, since the close of the year 1826, when he published his work on the subject:—no doubt it has become very considerable. But upon referring to that work, there is abundant evidence to show that the knife should never be resorted to in any case, unless

it be first ascertained that that method (or some other equally, or more practicable one *yet* to be discovered) cannot be available.

At the period just referred to, Civiale had operated upon 43 calculous patients, but one of whom died *during* the treatment necessary. Their united ages amounted to 2184 years, which shows an average of within a fraction of 51 years. The case that terminated fatally, we give in the words of Dr. Civiale:—M. Cornu, aged 72—disease of five years standing—treated during 3 months—2 voluminous and very hard calculi—bladder diseased—constitution very feeble—before the end of the treatment, M. Cornu, as a consequence of a deviation from the prescribed regimen, experienced all the symptoms of acute gastritis, which proved fatal. This was demonstrated by a post mortem examination. It was likewise discovered that the bladder *still contained a fragment* of stone, one inch and a half in its greatest diameter, and that this viscus was in the state in which it is ordinarily found, when the disease has been of long standing. With regard to the volume and texture of the calculi in all the other cases, there appeared to be such a variety as might have been expected, in such a number of patients. They varied from the size of a hen's egg, down. The larger size and greater hardness constituted, of course, greater difficulties, and required more frequent operations.

Perhaps it is fairer to test the advantages of this method by the first few years' practice, than by that of subsequent periods, as it was then novel, and no patients, but those whose chances of success by other methods were diminished, by the ravages of age, or by great constitutional debility, induced by their long standing calculous affections, or from any other cause, could be brought to submit to it.

It would be expecting too much of the method of Civiale, as of any other process that could possibly be devised, to suppose that it would be exempt from difficulty. There must of necessity be great difficulty, if not absolute impossibility, to grasp an extraordinarily large stone; and the perplexity of the case would be inevitably increased were it very hard.

Fragments too, as in the case of M. Cornu, sometimes remaining, and irritating the bladder, by their sharp angular projections, must be put down as an important point of consideration. If the stone be encysted wholly, or partially, this method is out of the question. Independently of the stone itself, great difficulties, must likewise occur, where there exists considerable morbid irritability of the bladder, and especially where such irritability pervades the canal of the urethra, or where there is a stricture in that canal.

The latter obstacles, however, may no doubt, be generally overcome by cautious and prudent management. With regard to the last, stricture of the urethra, we cannot but direct the attention of the junior members of the profession, to the wise, and prudent, and *slow*, and successful practice of the chief surgeon of the Hotel-Dieu of Paris, Professor Dupuytren, in such cases. He always reminded us of Old Fabius, who conquered by delay, whilst younger medical Varros sometimes ruin their cause by their haste and impetuosity. With regard to simple organic stricture, the usual mechanical method of bougies, of increasing diameters, successively worn in the urethra, for 15, or 20, or 30 days, commonly overcame them. Occasionally a bougie could not be made to enter the stricture; but upon being left in the urethra, the end of it in the neighborhood of, or touching the strictured part, it seemed, after some hours of *patient waiting*, to determine an expansive action in the tissue of the part, and *then* the bougie could be insinuated, and left there, to prepare the way for the next larger one. M. Dupuytren always employed, in these cases, bougies of gum elastic, of a conical form, decreasing to a very small extremity. The writer of these observations was frequently interested, in observing in cases of spasmodic stricture especially, the happy effects of the above prudent practice. Sometimes when the smallest bougie could not be made to enter the obstacle, the presence of even a large gum elastic sound for seven or eight hours, as before explained, would prepare the way for the subsequent introduction of the bougie.

Our object, in making the preceding remarks, is to explain

how morbid irritability, and even stricture of the urethra, may perhaps in most cases, be *slowly* overcome, so as not only to admit of the introduction of the straight sound, but to bear its presence during the operation. If we take this in connexion with the Egyptian practice according to Prosper Alpin, already adverted to, we know not to what extent the urethra might be dilated by *slow and cautious* means, for the introduction of straight sounds, of even larger diameters than those that have been hitherto employed. We would, in this way, account in some respect, for the failure of many attempts, that have been made in this country, to practice by the method of Civiale, in so much indeed, as to impair the confidence of the American profession, as to its extensive usefulness. It has, no doubt, sometimes happened, that after *hurried*, ineffectual efforts have been made according to that process, recourse has been eventually had to cutting, when judicious management might have insured success. The patients might have been in agony at the introduction and presence of the instruments in the parts, and might have subsequently yielded themselves to harsher treatment; but as these cases seldom call for prompt and rapid action, it might yet remain a question, whether nature might not have suffered herself to be *coaxed* by degrees, in her own way, into subserviency to our purposes.

Various modifications of Civiale's instruments have been proposed and even practiced with by others; but it does not come into our plan to enter into an account of them, particularly as (so far as they have come to our knowledge) they have been found to be less ingenious and less practicable than those of Dr. Civiale.

We will now briefly consider some only of the means that have been practiced to meet the second great indication, viz. cutting a passage for the exit of calculi. It was a part of our plan, to take a rapid glance at the history of *Lithotomy*, in order to bring into comparison the various methods that have at different epochs been devised and adopted;—to trace some of the more remarkable improvements that have from time to time been added to the resources of surgery, and especially the

improvements of later times;—to distinguish what in fact is new from what is old;—to collect together (as far as time, and the limited means within our reach of consulting the annals of medical history would allow) the relative average of successful cases in each mode of practice;—in a word, after a careful review of all, to sum up in favor of some method, or methods in particular, which might afford the best chances of safety to the patients themselves, and reflect most credit upon the profession. Time however, and the limits we prescribed to ourselves, would not admit of it.

As no operation has more exercised the genius of medical men in every age than that of lithotomy;—as the walls of the bladder have been attacked by instruments on all accessible points;—as the great embarrassment consists in the proper appreciation of the divers methods, so as to form a correct estimate of the circumstances in each case, that should determine in favor of one method in preference to another, or, if possible to ascertain whether *one mode* in particular, might not be found to combine within itself all the requisites for the generality, if not for all the cases, that *require* the aid of lithotomy;—such a research, comprising a review of all the questions bearing upon this department of our subject, constituting of itself a sufficiently extensive field of investigation, would, in the hands of some more skilful laborer in the vineyard of medical science, no doubt, be highly acceptable to the profession, and prove eminently useful to the sacred cause in which we are all engaged. We will confine our few observations to those operations that have been practiced in more modern times.

The high operation, or that performed above the pubis, was probably the mode employed by Colot, in the case of a criminal laboring under stone, as far back as the year 1475. It is reported, that the criminal recovered in 15 days, and was pardoned by Louis XI. But the first well authenticated operation was one practiced by Pierre Franco, at Lausanne, in 1560, on a child 2 years of age. It was a case of necessity. He commenced by the operation of Celsus; but the stone being as large as a hen's egg, and seeing the bladder cause a

considerable prominence above the pubis, he made the incision there. The case was successful, but he still advised others not to follow up such practice, on account of the danger, now always apprehended—*urinary infiltration.*

In after times the improved processes of Cheselden and Frere Come seemed, at least in their hands, to be sufficiently successful: for of 15 cases, in 4 years time, occurring in the practice of Cheselden and Douglas, but 2 patients perished; and out of 82 operations by Frere Come, almost all the patients were cured. In more modern times, however, the proportion of successful cases to those not so, appears to be only four to one. This operation does not present the danger of cutting any important vessels:—in upwards of 80 cases, Frere Come encountered but one single hæmorrhage. But if this advantage be taken into consideration, on the other hand, the patient is exposed to all the evil effects of urinary infiltration in the cellular tissue, and consequently of inflammation of the peritoneum, &c., accompanying it. At this time, the high operation is confined to those cases, where the extraction of voluminous calculi is thought to be impossible, by the methods employed below the pubis.

The common lateral method, we know, was first practiced by Frere Jacques, in a very rude manner, towards the close of the 17th century. Out of 42 cases at the Hotel-Dieu and 18 at the Charity hospital, making 60 in all, 25 patients died. In several of these cases he had cut into the rectum. This method, however, has successively undergone many essential improvements, in the hands of Rau, Cheselden, Bromfield, Frere Come, Hawkins, and others, and at this time, we believe, the number of deaths in Europe is about one in six: in America, however, from some cause or other, the operation seems to be less formidable in its character, as the mortality is evidently in considerably less proportion. The lateral method exposes less to inflammation than the high operation; but there is much greater danger of hæmorrhage. If the surgeon approaches too near the bulb of the urethra, he may probably wound the arteria transversalis perinæi. Should the incision incline too transversely, he may even run the

the risk of dividing the internal pudic artery. As for the superficial artery of the perinæum, frequently no precaution can save it. To this may be added, the danger of cutting into the rectum, when the incision is carried too far backwards. Young practitioners should be very careful of this latter circumstance, and should operate several times upon the dead subject, before trying their skill on the living. We remember well the *sad* accidents of this sort, that befel many of the *dead subjects*, in the practice of some of the members of the class of operative surgery, whilst we attended it under Professor Lisfranc, at the Pity hospital of Paris. In the lateral method the lithotome cache of Frere Come is now generally employed in France, and we believe, frequently in England. The easy and safe mode of introducing the lithotome, and then of directing the incision from within outwards; the perfect control which even an inexperienced hand may have over the instrument, certainly gives it decided advantage over the gorget, and should recommend it likewise to the American Profession. With regard to the two preceding methods, and the improvements that have been successively added to them, down to our own times, they are matters of sufficiently general medical notoriety, not to insist farther. Our object has been simply to lay down at first the advantages and disadvantages (according to the general consent of scientific practitioners) of methods still in use, but long known, and then rapidly to contrast them with the real, or supposed claims of methods peculiarly of our own day, and not so generally known.

A new mode of operating was proposed in the year 1818, and has since been practised with various success by M. Sanson, called the Recto-vesical method. But we are told at the outset, that we should call things by their right names, and style it the vertical method, or the grand appareil, or the method of Marianus Sanctus, of the sixteenth century, revived. That the external incision should be made along the median line of the perinæum in both the old and new methods, is only one point of resemblance, and cannot subject the author of the new one to assuming credit (whatever may be the credit eventually awarded to it by the profession) to him-

self for the labors of antiquity. Besides Marianus Sanctus did not in truth make his incision on the median line; but a little to the left of the raphe. The subsequent stage of the old operation, was simply to dilate at once, the neck of the bladder and the neighboring part of the urethra, to the degree necessary for the exit of the stone, and this, of course, could only be effected by the *laceration* of the parts. We need scarcely advert to the introduction of a grooved staff, or catheter to serve for the direction of the other instruments, as another point of similarity, as this is common to all the methods below the pubis of the present day. Surely no enlightened practitioner of the 19th century would wish, even if he could do so without detection, to encircle his brows, with the honor of having invented so cruel an operation. It suited the period of its vogue; because surgery could then offer nothing better.

The recto-vesical method of M. Sanson is simply this, a grooved catheter being introduced into the urethra, the operator makes a longitudinal incision along the raphe, commencing at, or near the anus, and extending forwards in length, of course, according to circumstances, and divides the membranous portion of the urethra: then introducing a probe-pointed bistoury into this opening, and directing the end of it along the groove, he divides the prostate gland, and the neck of the bladder and a portion of its inferior wall. The stone is extracted as usual. Sometimes the anterior and inferior portion of the rectum and the sphincter ani are involved in the incision, which, beyond all doubt, should be avoided, if possible. M. Dupuytren has several times practised this operation; but he employs the lithotome, making the incision of the neck of the bladder, &c., in withdrawing the instrument, which would we think always be preferable.

Professor Vacca Berlinghieri has slightly modified this process, and after the end of the bistoury has been made to enter to the distance of about one inch into the interior of the bladder; without changing the position of the catheter, he raises the handle of the bistoury to a distance, required by the necessity of the case, in the direction of the scrotum. By this

motion, the back of the blade resting upon the groove of the staff, the end of the bistoury, within the bladder, must separate from the groove, and the cutting edge of the blade must necessarily divide the neck, prostate gland, &c. hi; usl position of things, the bistoury is withdrawn. The recto-vesical method has been frequently practised in Italy, and, it has been reported, with considerable success. In France, experience had been too limited, when we left that country, to warrant an ultimate decision as to its merits. It will strike every medical reader, that the incision directed along the median line, goes out of the reach of any of the arterial trunks, and that, of course, there can be no fear of hæmorrhage. Practice has confirmed this advantage. Neither is danger from inflammation to be apprehended. These are weighty circumstances, that preponderate much in its favor over either the hypogastric, or lateral method. But its friends are not blind to its great and perhaps only radical defect, its exceeding liability to occasion urinary fistulæ; which cannot but be viewed as one of the most incommoding, nay distressing and, in many cases, permanent evils that could be consequent upon lithotomy.

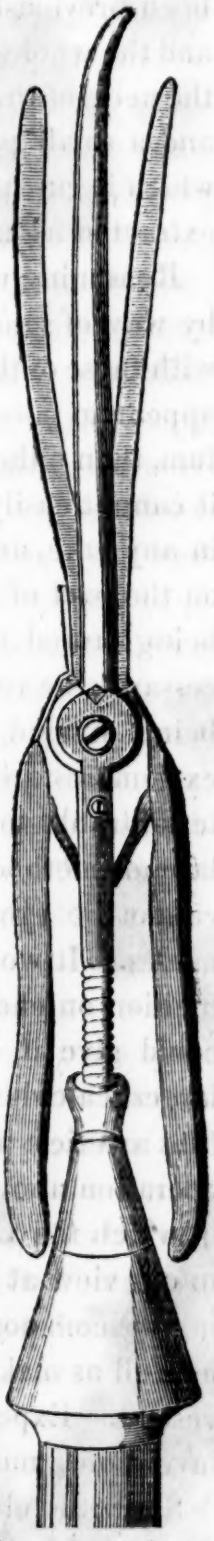
We will conclude our observations by noticing the transverse, or bi-lateral method, as practised by Professor Dupuytren at the Hotel Dieu, of Paris. Again must we name this the method of Celsus, rescued although it may have been indeed from the wreck of ages; because, forsooth, the incisions are transverse. But the Roman simply directed the surgeon to go forth to the aid of suffering humanity, armed with a bistoury in his right hand, and the fingers of his left. At the close of the operation, to be sure, if his fingers were inadequate to the extraction of the stone, a crotchet, of a peculiar construction, was to be brought to his aid. The Augustan age had, however, been stripped of its laurels by MM. Chausier and Ribes, already in the 19th century, (1805) and Beclard had practised upon the living and the dead as late as 1813. It had been, at the first period, forseen that a conductor was only wanting to direct the incisions with safety, that the Celsian mode might become a good one even in modern times.

After the first incision (the catheter being previously introduced) the urethra was to be cut into as usual—then the neck of the bladder and prostate gland were to be divided, on one side *first*, and *then* on the other—or indeed both might be divided at once with a double lithotome cache, like that of Fleurant,* more anciently described by Pierre Franco. As far as our limited research could extend, we have not been able to ascertain the fact, that Pierre Franco ever did invent a double lithotome cache. It is certain that he did, however, propose a single one, to all intents and purposes similar to that of Frere Come. We must then cease calling this last instrument by its accustomed name, and style it the lithotome cach of Pierre Franco, because, although it had slept in quiet oblivion for two centuries, it still owed its discovery to him. With about the same reason would we fasten upon the distinguished individual who has added to the materia chirurgica the beautiful instrument which now goes under his name, the surgical plagiarism of having taken credit to himself for the crude and undigested labors of long past, as well as of recent times. Whatever may be the merits of the question, these late developments seemed, even in the short period up to 1824, to have been already withering under the wasting influence of premature old age. Even the keen and searching eye of a Dupuytren had not been able to penetrate through the thick shades that had already begun to invest them with their oblivious mantle, even in the city of their birth. The vigorous intellect of that man of the age, it appears was wanting to act upon the same subject—his ingenuity, and skill, and dexterity, were to be exerted to give it celebrity, to convince men of its practicability, its safety, its usefulness; or the world might have rolled on for two centuries more, before some fortunate *discoverer* might have illustrated his name with the labors of the beginning of the 19th century.

The instruments required in the method of Dupuytren are, an ordinary grooved catheter, a bistoury, and a double lithotome cache. The form of this last nearly resembles that of

* Richerand Histoire des Progrès Récens de la Chirurgie.

Frere Come: it has, however, two blades, curved on their sides, and which, lying upon each other, and concealed in the body of the lithotome, are prevented from wounding the canal of the urethra, &c., when the instrument is introduced. These are made to separate from the body of the instrument by pressing two levers, that extend backwards on each side of the handle, to the handle itself. The handle is of a conical form, and is moveable on a graduated screw, and so disposed, that the separation of the two blades can be regulated at will. The cutting edges of the two blades, during the separation, describe also a slightly curved direction, so that the whole incision will have a curvature, whose convexity will look upwards and forward. In the procede Dupuytren, as in every other, the operator must be provided with director, forceps, &c.



The manner of conducting the operation is this: the patient being placed and secured in the position required for the lateral operation, a grooved catheter is introduced, as in the ordinary way also, and kept in a vertical position by the assistance of an aid. With his bistoury the operator makes, across the raphe, about half an inch above the anus, a transverse incision, of about half an inch in length, with a slight curvature, whose concavity looks backward, and divides the skin and cellular tissue. A second incision divides the inferior wall of the urethra. The bistoury is then laid aside, and the extremity of the lithotome is made to pass through the opening of the urethra into the groove of the catheter, and along the course of this into the bladder. The catheter being now withdrawn, the lithotome is held in such a manner that the concavity of its curvature looks downwards and backwards, which was the

very reverse in passing the instrument into the bladder. The two levers are then pressed against the handle, which has been previously turned to the requisite degree on the screw, and the whole instrument is withdrawn, cutting on each side of the neck of the bladder, the two lateral lobes of the prostate, and a small part of the membranous portion of the urethra, which is cut after the fashion of a pen. The stone is then extracted as usual.

Reasoning upon the prima facie evidence of the operation, by way of summing up its probable advantages, in comparison with those of the other modes already remarked upon, it would appear to possess a much greater tendency to spare the rectum, than either the recto-vesical, or lateral method. In fact, it cannot easily be conceived how the rectum can be reached in any case, unless it should arise from absolute carelessness on the part of the operator. The convex side of the blades being turned upwards and anteriorly, the incision must necessarily be removed from the rectum. The whole incision being divided between the two sides of the median line, its extremities are but slightly removed from it; hence the arterial trunks might be considered in less danger than in the lateral method. The danger of consecutive inflammation cannot be greater than in either of the two above-named modes. It would appear to follow, from the division of the incision on each side of the median line, that the operator could give it sufficient extent to afford a passage for the largest calculus, without running much risk of reaching important arteries; and hence, the necessity of ever recurring to the operation above the pubis might be obviated. The manner in which the incision is made from within outwards, would, to our view at least, seem preferable to the mode of conducting the common lateral operation with the gorget, or bistoury, as well as making the incision with a bistoury in the recto-vesical. Experience, however, the great arbiter of all human inventions, must also determine in this.

Since the close of the year 1827, we have not ascertained what has been the voice of that experience. Previous to that time, however, whilst we were attending the clinical instruc-

tion at the Hotel-Dieu, of Paris, a rule had been established in that hospital, of submitting the calculous patients alternately to the three methods below the pubis, with the view to test their relative superiority. There could, in this way, be no choice of patients for any one. Before we left the country, the whole number operated upon under that rule was 21, among whom there was such a variety of age and circumstances of constitution, as might have been expected. It had been agreed that M. Dupuytren should employ the transverse; M. Sanson, second surgeon of the hospital, the recto-vesical; and M. Breschet, surgeon in ordinary to the same establishment, the common lateral method. Out of the 7 cases operated upon according to each process, the transverse was attended with entire success; 2 patients perished that had been submitted to the recto-vesical, one of whom (65 years of age) had been, a month previously, subjected to the process of lithotrite by M. Meirieu, all the other circumstances of his case were, to the last degree, unfavourable; M. Breschet lost one patient. Previous to the commencement of this trial, M. Dupuytren had performed the transverse operation two or three times with complete success.

Reviews and Bibliographical Notices.

Art. V.—Certainty in Medicine.

An Essay on the Certainty of Medicine. By P. J. G. Cabanis. Translated from the French, by R. La Roche, M. D.

BY A CORRESPONDENT.

It must be evident to every competent observer, that the medical profession in many parts of our country, is strikingly deficient in the talent and acquirements which the importance of its duties demands. If the art is but too often the subject of ridicule on account of its inefficiency; if physicians have failed to obtain that respect to which their high functions entitle them; if impostors are patronised to the mortification of the regular practitioner, and lying panaceas *not seldom* preferred to judicious prescriptions—the profession may look to itself as a cause, in a great degree, of such evils. Is it not the case in many instances, that physicians, after *finishing their studies*, as it is termed, and commencing practice, abandon whatever *science* they may have attained, and sink at once into mere empirics? Hear them railing against books, and theories, and speculations, and dwelling forever on the exclusive efficacy of individual experience, and you would be apt to imagine that they had joined hands with impostors, to pull down the useful, though still imperfect, fabric of medical science, the establishment of which has called forth, for more than three thousand years, the exertions of some of the most philanthropic and highly gifted individuals. The misfortune is, that the empiricism of a regularly bred physician, is of the most pernicious sort. Ordinary quacks borrow from each other, or adopt into their system remedies sanctioned by the experience of the people, or inherit from predecessors, medicines whose worth long use is reported to attest; but our extraordinary quack shuts his eyes to the light of others' experience; establishing a routine for himself, he never departs therefrom, or revises it by the aid of others' knowledge, to see whether it may not be improved.

Many physicians, without acting so blameably, never come up to proper ideas of the art of physic. Their conceptions of the subjects of medical investigation are confused, and their views of the field of medical research but limited. Neither are they aware of the peculiar difficulties that encompass

their art, nor of the methods they must adopt to overcome them. Now it is evident, that contracted notions of the subjects, and ignorance of the difficulties of any science, lead to but meagre and mistaken efforts for its advancement. Hence the vast utility of those works, which, though not strictly practical in their character, give us comprehensive and clear views of the boundaries of an art or science, and point out the best methods of its cultivation.

The essay of Cabanis, on the certainty of medicine, is a work of this kind. The object of the author appears to have been, to establish the claims of medicine to the rank of a science, vindicating it against the smart sayings of wits, and the grave accusations of philosophers. The art of physic, he says, has its foundation in nature, in the wants and instincts of the animal economy. All mankind are liable to pain, disease and death. The sick man seeks to be cured, pain cries for relief, and death is the universal dread. Medicine informs us of all those agents, accumulated by the experience of the world from its beginning, which are calculated to assuage suffering, restore health, or prolong life. Wits and philosophers, therefore, who ridicule and asperse the healing art, only manifest their folly and ingratitude, unless, indeed, they be exalted above the common ills of humanity. They abuse their only hope of safety.

Cabanis, in using the term certainty, as applied to medicine, distinguishes it from the certainty accompanying a mathematical demonstration. It means nothing more with him than a high degree of probability—a probability of the same sort and degree as those which regulate us in the ordinary business of life. It appears to us, that in his zeal to raise medicine to its proper place among the sciences, the author has claimed more for it than can well be substantiated. If we mistake not, he supposes medical science capable of being carried to the same perfection, or rather, admitting of as much certainty, as the other departments of knowledge dependent upon observation. We beg leave, on this point, to differ with our author. In our inquiries into the nature and phenomena of living bodies, we must be content with a far less degree of probability, than in the sciences which treat of the affections and circumstances of inanimate matter. A general reason that may here be given for this fact is, that the phenomena and principles of living bodies are far more complicated and hidden than those of inanimate matter. A more particular reason, in relation to the art of curing disease, we will give presently.

Cabanis, by way of establishing the certainty of medicine, notices, and answers in detail, the various objections of cavil-

ers against our art. These objections are, 1st. Our ignorance of the secret principles of life. 2d. Our ignorance of the nature and proximate cause of disease. 3d. Our ignorance of the essential nature of curative agents. 4th. The want of unanimity in the opinions and practice of medical men. 5th. The extraordinary abilities of every kind required in medical researches. 6th. The complication of diseases, and their modifications from almost innumerable causes. 7th. The variations of remedies in their action, from circumstances unknown to the physician. All these objections, except the two last, the author has answered satisfactorily. These two, we think, he has failed to remove, not from a want of ingenuity, but from the very nature of the case. Including them under one head, we shall endeavor to show their real force.

The perplexing intricacy, complication, and deceptiousness of medical phenomena, must certainly qualify our opinion concerning the certainty of medicine. And first, as it regards etiology. The physician, in the course of his practice, meets with a disease immediately preceded by a multiplicity of circumstances, all of which may have morbific tendencies. His business is now to determine, whether one, several, or all of them, have been instrumental in the production of the malady before him. He should also ascertain their individual power, and modes of action in a separate state, and what modifications they undergo in combination—how far they may counteract or favor each other. Next, their mode of action on the animal economy is to be inquired into—as to the parts or structures they primarily affect, and the peculiarities of the functional disturbances they occasion. Nor must he neglect to examine what circumstances, whether extrinsic or pertaining to the system itself, prevent, mitigate, or enhance, their morbific impressions. He must, indeed, be dull sighted, who does not at once clearly perceive that all these phenomena are so darkly complicated, so intimately blended, the most acute and patient observation will be puzzled in analysing the complex mass, discriminating between appearances and realities, and tracing out individual facts. Facts in pathology are equally difficult to determine. To this day, the profession is not agreed as to the first link in the chain of morbid actions; or as to the order in which they occur. And who is ignorant of the fallacies attending post mortem appearances? The difficulty of discriminating, at times, between congestive phenomena, and the marks of inflammation; of determining whether the morbid local appearances have preceded or followed the general malady; and in fine, of drawing an unerring and universal line between healthy and morbid post-

mortem conditions—is only equalled by the dogmatism with which theorists and hasty observers have settled all these questions. The symptoms, or outward manifestations of diseases, are also peculiarly perplexing. It is true, accurate descriptions of most maladies have been abundantly written; but how imperfect, after a lapse of nearly three thousand years of observation, is our diagnosis. The knowledge of the essential and peculiar signs of a disorder, is almost in its infancy; and the continuing variety of opinion on this subject is a sufficient evidence of the difficulty of its investigation. So hard is it to disentangle the complex mass of symptoms, and refer each to its proper source, and so deceptive are the characters of disease, one malady affecting at times nearly all the signs of another—giving rise, by various sympathies, to contradictory indications, or becoming complicated in its course with another affection, which also has its corresponding manifestations—that the physician must either have had extraordinary skill, or prodigious vanity, who has not often exclaimed, who is sufficient for these things?

But it is in therapeutics, or the application of the materia medica to the cure of disease, that the deceptiousness of phenomena is most remarkable. The comparative influence of art and nature on the termination of disease, and the ascertainment of the efficacious remedy, where several medicinal agents have been simultaneously employed, are the points, concerning which we are most likely to be deceived. Ascribing to medicine what nature has performed, and giving exclusive credit to one article for virtues which result from its combination with others, or pertain, it may be, solely to them; are exceedingly common faults in our profession—errors, indeed, which cannot always be avoided. Many other sources of misconception beset therapeutical researches. Age, sex, physiological condition, and peculiar circumstances attending the administration of an article, exert a strongly modifying power over medicinal properties, and yet how often are they forgotten! The remedy, too, may be spurious, or it may have lost its activity. There may have been some error in the time of collecting or mode of preparing it, in the manner of preserving it or method of exhibition. Above all, idiosyncrasy is a never-failing source of fallacy, not only in therapeutics, but in all inquiries where the action of agents upon the animal economy is to be determined. Medicines effective on one individual, may be inefficient on another. A cause of disease on one will fall harmless in another. Nor can these idiosyncrasies often be detected by observation. There are no external signs, no indications whatever, to advertise the mind of their existence. When we prescribe a medicine, it

must act in subjection to circumstances we cannot exclude, over which we can exert little control, and whose influence cannot be exactly pre-estimated. For the same reason, the power of a morbific agent to produce disease, cannot be measured. It is complicated, too, with other causes, and is subject to influences we can neither abstract, nor accurately calculate. In physics, on the contrary—for example, chemistry and mechanics—we can exclude from an experiment circumstances that would modify the result, or at least estimate the amount of their influence.

Two other circumstances, which Cabanis has neglected to notice, might be adduced as strong objections against the certainty of medicine. We will introduce them, not indeed as affording sufficient reasons for disbelieving in the efficacy or excellency of our art; but as being necessary towards forming a tolerably correct idea of the nature of medical evidence.

The first circumstance is, the imperfection of language, arising from the intrinsic nature of the subjects of medical science. Without deriving our illustration from physiology, or from the theory of disease, in which the use of hard words, without meaning, seems legalised, and where the imperfect language is not unsuited to imperfect ideas—we may refer to the description of the signs of disease, and to its nomenclature. He must, indeed, be a novice, who would think of learning the manifestations of a malady by description, or its nature by nomenclature. The attempt to divide, and subdivide, to generalize and specify, disorders, is justly discarded by many of the most enlightened of the profession. Our knowledge of the distinctive characteristics of different diseases is as yet too meagre, and the line of demarcation between them too finely drawn, (they being strangely blended and complicated with each other) to allow, at this stage of the science, an adequate and a correct nomenclature. As regards the description of disease, it is certainly all right, though not sufficient. For after learning all we can from books, the information is almost useless without eye-sight. It may, indeed, *aid* us in recognising a malady, when for the first time we meet it in practice. But how different are our ideas then, from those we have derived from the mere description. The peculiar, striking traits, or diversities of aspect, presented by different disorders, can no more be depicted in words, than can the characteristic expression or cast of countenance of an absent person. This is why the student is directed to the wards of the hospital, or chamber of sickness; and this, too, explains why the young practitioner is so often bewildered, utterly confounded, at actually encountering, for the first time, a

malady, with which in books he had been long familiar.

The second circumstance is, that the nature of medical phenomena scarcely ever admits of experiment. "Experiment," says Professor Leslie, "consists in a kind of trial, or artificial selection and combination of circumstances, for the purpose of searching after remote results." From the terms of the definition, it is easy to see the inapplicability of experiment to inquiries in medicine. In physics, chemistry for example, we do not have to wait for the spontaneous recurrence of a phenomenon, to determine its nature or cause. Experiment enables us to anticipate the results of mere observation.

Guided by certain analogies, or perhaps by no analogies at all, we continue numerous and diverse combinations of circumstances at pleasure, until at length we create the hoped-for phenomenon. And now able to reproduce it when we please, and under various modifications or varieties of circumstances, we at last determine its cause. We cannot so proceed in medicine, men may give themselves to Satan for money, but scarcely their living bodies to the doctor, to advance medical science. We cannot persuade people to submit to the action of various combinations of morbific causes, for the sake of a more accurate etiology; we cannot reproduce a disease at pleasure, that we may more correctly determine its symptoms; we cannot at the first onset of a malady, dissect the live body, that we may detect the structures that are affected and ascertain whether certain inflammations really precede or follow the general morbid action. In remedies, the physician does indeed sometimes experiment, but it is only when he knows not what else to do—not then for science's sake, but for his own.

The materia medica, to a certain extent, is susceptible of experiment. We may try upon ourselves or upon animals, the action of a new article; but such experiments, however numerous, yield but probable inferences. The influence of the article upon a dog may differ vastly from that it will exert on a man—its action upon my system is no exact standard for estimating its action on another. Nor are its effects upon the animal economy in a state of health, sure data for determining its effects upon the same in a state of disease. That the experiment may be satisfactory, the article should be exhibited, as recently or long prepared or collected—in different doses, every form, and diverse combinations; to numerous individuals, of opposite constitutions, and every variety of idiosyncrasy; under the varied circumstances of health and disease; at different seasons and in different climates. All this, it is evident, cannot be compassed by experiment;

at least by any other experiment than such as may be made use of by many individuals, at distant periods, and under disimilar circumstances, in the course of their observations.

Analogy too, may furnish, in materia medica, many useful suggestions, but never certain information. Analogies drawn from botanical likenesses, or similarity of chemical composition, may lead to the notion of a resemblance in medical virtues; but the notion must be verified by experience, ere it become a principle of practice.

Our knowledge must therefore be built chiefly upon observation. This, of necessity, renders the art of physic, one of slow progress; and at the same time, our knowledge imperfect and uncertain. An age of close observation may be required to determine that, which experiment, were it in our power, would determine in a day. We might here, for the sake of illustration, adduce various instances exemplifying the tediousness and uncertainty of the method of observation, as necessarily practiced in medical researches—but it is presumable, that no further elucidation of the subject is needed.

It must be recollected, that what we have said, applies only to the doctrine of the cause, nature and cure of disease. Several depatments of knowledge more or less subsidiary to medicine, admit to a greater or less degree, of the use of experiment. Physiology is almost, and chemistry altogether entitled to the name of an experimental science. No improvements in chemistry, however, no additional anatomical or physiological knowledge, and no discoveries in botany will ever, of themselves, greatly advance the art of physic. This, now as ever, we repeat, mainly depends on accurate and long-continued observations of the relations of certain agents to the animal economy.

We had written thus far, when our attention was attracted accidentally by the following remarks of a late writer, on the comparative powers of experiment and pure observation, as methods for the discovery of truth. "By means of experiment," he says, "a kind of practical induction is carried on, almost visibly submitted to the perception of the senses, which promotes and suggests conclusions, as it were, mechanically, and checks the errors to which the abstract operations of the mind are constantly exposed. And in the second place, the great and leading advantage of experiment, is the power which it bestows of creating facts, as it were, at pleasure, by which means the mind may select only those luminous facts, called by Lord Bacon prerogative instances. A few of these, he tells us, speak more plainly than a multitude of common facts, so that a consequence may be

deduced indirectly from the comparsion of a few examples, by a simple intuitive act of the judgment.

When pure observation becomes exclusively the purveyor of our knowledge, the case is far otherwsie. Here, we must accept, without choice or selection, whatever nature or accident may please to present. There is no power of varying their condition or of changing their combinations,—no means of decomposing them practically, of adding a circumstance, or of withdrawing a circumstance, and observing the changes which follow."*

The foregoing remarks will perhaps tend to show still more clearly, how much medicine has suffered from the impossibility of practicing experiment, and how uncertain, in consequence thereof, is its evidence.

If our observations be correct, it is evident that Cabanis claims too much for medicine, when he supposes it capable of the same degree of certainty, as the natural sciences generally. But is this any reason why the art of physic should be stigmatized as a useless or pernicious art? Then for the same reason, we may discard ethics, the art of government or political economy as useless. Every day we perform many actions from lower probabilities, than the probability of cure, which physic holds out to the sick man. And if such conduct be reasonable in us, is it not madness in him to reject the proffered aid of medicine? "In questions of difficulty," says Bishop Butler, "or such as are thought so, where more satifactory evidence cannot be had, or is not seen, if the result of examination be, that there appears upon the whole, any the lowest presumption on one side, and more on the other, or a greater presumption on one side, though in the lowest degree greater, this determines the question, even in matters of speculation; and in matters of practice, will lay us under an absolute and formal obligation, in point of prudence and interest, to act upon that presumption, or low probability, though it be so low as to leave the mind in very great doubt which is the truth."† Now medicine may be considered as a collection of probabilities in relation to the causes and remedies of disease, in some cases amounting to what Cabanis calls "*practical* certainty," in others constituting but a low presumption, but altogether entitling the healing art to the respect and confidence of mankind. "Far from being," says our author, "as asser-

* Dr. Tod's Book of Analysis, p. 17.

† The Analogy of Religion, Natural and Revealed, &c., by Joseph Butler, LL. D. &c.

ted by some exaggerated minds, the scourge of humanity, the science of medicine is, on the contrary, to be regarded as its hope, its safeguard; and promises in future, to present its resources, which from day to day will acquire greater extension and more efficacy." Nor is this a rash hope. Our science, despite all difficulties, has been steady, though slow in its growth, and now, under more favorable auspices, bids fair to become a magnificent tree, whose branches shall fill the earth, and of whose healing fruit, mankind, with diseases of whatever name, will taste and live. The numerous facts, that have been accumulating for more than two thousand years, have already begun to yield the rich fruits of important principles. A few great laws have been educed from the chaos of the phenomena, enough to redeem medicine from the charge of being altogether empirical, and giving us ground to hope for still more important and useful generalizations.

"I dare make bold to predict that, together with the true method of observation, the spirit of which should always predominate in it, will soon revive in medicine, and that the science will assume a different aspect. The various fragments of which it is now constituted, will be assembled, in order to frame with them a system, simple and fruitful as are all the laws of nature. After a faithful examination of all the facts, and after they have been verified and compared, they will be linked together, and referred to a small number of principles, fixed or susceptible of little variation. The method of this investigation, of uniting by their analogies, or differences, of deduciug from them general rules, which will, in fact, be but a more precise detail of them; this method, I repeat, will be carried to a higher degree of perfection. The important and difficult art, that of applying these general rules to practice, will be simplified." Enough of this bold prediction has been verified, to save the prophet from the charge of presumptuous enthusiasm; and to give us grounds for believing, that medicine is far from having attained that degree of certainty, of which it is capable. Besides, we are to remember that many of the impediments to the progress of medicine, are not only not necessary, but may readily be removed; being dependent upon the errors of physicians, and not the peculiar nature of the art. By noticing some of these errors, the attention of the profession may be awakened to the importance of their rectification; and at the same time we may further the object of our author, which is the advancement of medical science.

1. We note first a want of exactness in following original, valuable recipes.

This is a very common error in our profession, and was long ago noticed by Lord Bacon as an impediment to medical learning. A reputable author may set forth a certain combination of articles, as particularly beneficial in a disorder; but the student does not hold himself bound to remember the proportions of the ingredients. Give me their names, he cries, and I will combine them according to general principles. It is not the fault of the student alone. The enlightened practitioner, misled by the convenient theory of treating disease according to general principles, is apt frequently to deem it a sort of quackery, to adopt precisely the prescriptions of others, however strong the evidence in their favor. He cannot believe medicinal virtues, to depend quite so largely upon the proportions of a mixture, and, in some cases, he takes the liberty of discarding what he conceives a trifling or inefficient ingredient. Now this is altogether wrong. Does he not, in a manner, insulate himself by such a course—cut himself off from whatever advantages are derivable from the multiplied experience of many—and foolishly confine himself to the narrow circle of his own experience? Who does not know, that notable virtues sometimes result from peculiar combinations, even though one or more of the ingredients may be separately inert—virtues indeed, which cannot be obtained by any mode of uniting the active articles alone. Nor is it rare, to meet with a compound whose efficacy is dependent upon the peculiar *proportions* of the ingredients. Dover's powder furnishes a striking exemplification of both these facts. One of the constituents, the sulphate of potass, is, by itself, comparatively an inert substance, in the proportion it holds to the other ingredients. Reasoning from this fact, a priori, we might readily conclude that the mixture would be as efficacious without as with it,—but experience has shown this to be fallacious. There is no combination we can make of ipecacuanha and opium, so excellently sudorific as Dover's powder. And not only so, but any deviation from the established proportion, is apt to diminish the efficacy of the compound. That the same is true of many other combinations, can scarcely be doubted. When, therefore, a person of sufficient judgment and experience, recommends a prescription as possessing peculiar medicinal qualities, we should forbear to modify it at our will, and then because unsuccessful, declare it of no utility. Nor, judging only from the properties of the ingredients in a separate state, should we pronounce it worthless. It would be wiser to employ it in the exact proportions, advised by its author, in the same doses, with the same precautions, under the same

circumstances every way,—and then not once, but several times, until we have determined its true worth. In this manner alone, can we judge righteous judgment, and hereby also, we shall avoid in one way, retarding the progress of medicine; for next to embracing the results of fallacious experience, the neglect or rejection of the fruits of correct experience has injured our art.

2. A kindred error, and one frequently giving birth to the fault just noticed, is a disbelief in specific remedies, and a consequent lack of inquiry for them. This we take to be a serious error, and a great impediment in the way of our professsion. So far as our knowledge now extends, there are few remedies of specific power,—a power, I mean, capable of making peculiar impressions on the animal economy over and above their stimulating or sedative influence. With the exception of calomel, iodine, sulphate of quinine, secale cornutum, and perhaps one or two more, our present materia medica may be divided into mere stimulants and sedatives, acting either generally or upon particular parts. Even the specific power of calomel and iodine have been called in question. (It will be recollected, that we use here the term specific, not as applying to an influence directed upon a particular organ, but to a peculiarity in the mode of action.) Sulphate of quinine, probably, is an article which cannot, without absurdity, be denied the possession of specific properties—properties whose impressions upon the living body cannot be supposed of a merely stimulant or sedative character.

The reason of this peculiarity of our materia medica, is to be sought for in the prevalent doctrines concerning the nature of disease. We have been taught to believe, that every deviation from a state of health, consists essentially either in a diminution, an exaltation or inequilibrium of the vital energies. Hence, the inquiry has been for remedies, that would meet these conditions, subduing, augmenting, or equalizing excitement, according as the case may be; and we have neglected to trace further their operation. Hence, too, the exclusion of specifics. Now we are among those, who disbelieve in the universal truth of these fashionable theories of disease. It is not our business here, nor have we time, to go into an examination of them; but we will briefly state the chief grounds of our disbelief. 1st. There may be exaltation, depression or inequilibrium of the vital powers, without disease.—2nd. Mere differences in the intensities of these conditions, do not appear to us sufficient, to account for the innumerable sorts and varieties of morbid action.—3d. The total in-

efficiency of stimulant, sedative, and revulsive agents, in many maladies, even when most judiciously administered, can only be explained by supposing different disorders to possess specific, essentially distinct natures.—And 4th. The diverse products, or terminations of disease, point to radical differences in kind as well as degree of morbid action. If this doctrine be true, and if, as is the case, we possess but three or four remedies able to exert a specific action on maladies,—to meet diseased conditions with corresponding antidotal powers—radically to affect and peculiarly to restore alterations in the vital properties,—how exceedingly deficient is our materia medica, how devoid of real strength is our art! Do you not see, that we are compelled to combat disease, almost without arms? Physic, says one, is the art of amusing the patient, while nature cures the disease: If he had said, the art of palliating symptoms, while nature cures the disease, his satire would have been no less true than severe. For certainly with our present remedies, we are scarcely able to do more wittingly than obviate the immediate effects, without reaching the hidden source and nature of disease. But this is our own fault. Influenced by famous theories, we have neglected to search out remedies, not sanctioned by them. If we have no certain, specific medicinal substances, it is not because they do not exist. Heaven has never afflicted mankind with peculiar evils, for which it has not provided appropriate remedies. The example of sulphate of quinine and the few other articles we have, possessing peculiar properties, teaches us to confess the reasonableness of belief in specific agents. Nor is there quackery in the idea, for the knowledge of them is to be gained only by long-continued and accurate observation; not by signs, and spells, and guessing, and the other expedients of imposture. Medicine, we believe, is destined to receive some of its greatest improvements from the search after specific remedies in relation to disease;—and a disbelief in their existence, has been among its chief impediments.

3. A third impediment arises from the absurd speculations of men of genius. Theorists have ever been busy in our profession: what pity that the judiciousness of their efforts has not equalled the ambition that prompted them. As it is, their labors have generally cumbered the science, whose march they hoped to forward. A man of genius, as ordinarily understood, will seldom stoop to the drudgery of collecting and examining facts. His vivacious imagination catches at a few phenomena, and lays them down as a foundation for an unsubstantial fabric of speculative principles. We are all addicted

to theorize, and can hardly look upon insulated circumstances, without directly seeking out some general laws which may connect and explain them. A theory, though untrue, is said by some to be beneficial,—acting as a stimulus to inquiry, and serving to give order and perspicuity to what facts may be collected. Theory, we answer, takes so firm a hold on the affections, as to alloy the love of truth and bias inquiry—impelling us in search of circumstances to confirm our hypothesis, and concealing from us or distorting those which oppose it. Buside this general way in which false theories act, they also retard the progress of physic by turning the attention of medical men from facts to words, from the investigation of nature, to the disputes of dogmatists. The distinguished theorists themselves, fascinated by their own chimeras or lusting after the praise bestowed on the leader of a sect, are no longer content to be the interpreters of nature, but would be sole directors of public opinion. Metaphysical subtilties, not tangible truths, are the proofs on which they rest their speculations. Inferior minds, enslaved by their authority, blinded by their ingenuity, perverted by their sophistries, or gratified with an opportunity for discontinuing painful research, build up the mass of error, whose foundations their superiors have laid. How great has been the waste of intellectual energy, in the attempt to resolve the multifarious phenomena of medicine into a few elementary truths, with the illusive hope of giving it the form of a demonstrative science. We have already had occasion to notice the evil influence exerted by the prevalent pathological doctrines. As further exemplifications of the pernicious effects of false speculations, we may point to the one-sided, imperfect investigations, which sprung from the notion of Galen, who derived all medical virtues from four elementary or cardinal qualities, heat, cold, moisture and dryness—from that of Stahl, whose practice, consisting, chiefly in a patient waiting upon nature, was felicitously styled, a meditation upon death—and lastly from the mechanical theories.

4. An impediment, nearly allied to the last, consists in the hasty, inadequate observation, and rash, unwarranted generalization of the profession at large. He observes hastily and inadequately, who does not carefully view a phenomenon on every side, and in all its relations; or who esteems that a fact, which, if inquired into, turns out fallacious. He generalizes rashly, who, from a few circumstances, infers a general principle or law, which he extends to other circumstances, connected with the first only by some faint or fanciful analogies. It is owing to the former of these faults, that our journals are so continu-

ally flooded with accounts of new, certain, all-powerful remedies, whose worthlessness, however, further experience soon demonstrates. Unwarranted generalization is the source of the vast number of crude theories of perennial growth in our art. It is easy to understand how these faults have retarded medical science. Incorrect statements of phenomena are so many obstacles to be cleared away from our path; and as to rash generalization, its evils have already been pointed out, when speaking of the influence of false theories. All we have further to remark is, that the error is not confined to men of great name. A physician having but three patients per annum, is not often at a loss for some general theory of disease, or operation of medicine; or without some notable remedy, which is made to perform as much service as the warm water of Dr. Sangrado. No matter whether it be a great or little genius who thus generalises, they equally cling to their darling heories; are equally eager in hunting up facts to give them countenance, and equally loath to receive witness against them. How much has medicine suffered from this species of egotism! The two faults we have just noticed are founded very frequently upon the ignorance or inability of professional men.

5. This leads us to consider a fifth impediment, which is the want of a suitable preparatory education among medical students. We might here take occasion to censure, what has so often been censured, that strange misconception of some parents, which induces them to select the most unpromising of their offspring for our profession. The lad of brilliant genius is destined for the bar; he, of the solid parts, is kept at home to manage the estate; commerce claims the quick, shrewd, and enterprising one; while medicine must be cumbered with the blockhead. This mistake can only spring from the grossest ignorance of what talent our art requires. Such parents would do well to recollect in time, how many lives will, in all probability, be sacrificed by their hopeful progeny, ere he learns that, than which *he* can learn no more, how to avoid doing mischief.

We say, that the preparatory education of medical students, is generally ill adapted to qualify them for the successful prosecution of their art. Our notice of this point shall be brief:—we mean not to say, that a student should have previously accomplished himself in the knowledge of the classics—of Latin, Greek, or other tongues. It is not tongues, but minds, that are needed in our science. Love of truth, a capacity for correct observation, and the power of judging good judgment, are the qualities necessary in physic. The first quality is re-

quired to keep the mind free from every undue bias; the second, to enable it to collect a sufficient store of available facts; the third, to guard it against deception, to trace the true analogies of phenomena, and infer general truths. Now whatever studies or pursuits are best calculated to endow the mind with these qualities, should be embraced in the preparatory course of the student. Hence the village rustic, who has habituated himself to the observation of human character, and judgment of human actions; or the farmer's boy, who has learned correctly to observe the varieties of soil, and to infer general truths concerning their relations with different productions; has acquired greater abilities for medical research, than the merely learned youth, who can read fluently, in the original, the Georgics of Virgil or Satires of Horace, but without appreciating the wit of the one, or wisdom of the other. We repeat, it is not knowledge, so much as power and discipline of mind, that the medical student requires, in order to make him an efficient member of his profession. Is there not now, has there not been a lack of such preparatory education? And has not the profession suffered severely from members devoid of the requisite abilities? Now, in order to secure the art against such artists—to save the field of medicine from the cumbersome, profitless tillage of such laborers—what is to be done? Let those whose business it is to examine into the pretensions of candidates for graduation, institute a more rigid, searching ordeal. The want of this, permitting the too easy admission of many, who can add neither glory nor benefit to the profession, constitutes a sixth impediment. For proof of this fact we appeal to the experience of the profession. Has not each of us known many a regular, diplomatized physician, whose claims to the confidence of the community our hearts have denied—who has excited in us wonder—wonder that ever a board of examiners, impartial and enlightened, should have styled him "Doctorem in arte medendi—dignum amplissimis honoribus," &c. &c. It is not our business to assign the cause of this—sufficient is it to know that the laxity of examinations is a radical evil in the medical system of this country; and the example of reform must begin in some high place ere the evil will be remedied. As it regards a man's qualifications for the *practice* of physic, we do not believe they can be ascertained, except by questions propounded upon disease before him. It should be determined in the wards of a hospital, whether the student can judge sufficiently well of the symptoms, complications, indications, and particular remedies of the maladies immediately under his notice.

7. A limited view of the subjects of medical investigation, and a foolish, vain confidence that all is known that may be known, have retarded our art, inasmuch as they withdraw all stimulus to inquiry. The remark is made in special reference to novices—to those who have but just stepped into the profession, with their young, blushing honours thick about them. I have heard some of them declare, they now knew as much as any one could teach them. All the treasures of medical wisdom and knowledge they seemed to fancy their diplomas had conferred. It is precisely a small mind that grows so suddenly all-wise and self-sufficient. An opposite error, but one whose influence is equally hurtful, consists in regarding as unascertainable the objects of medical inquiry; or being paralyzed in resolution by the vast difficulties that beset them. But we will not dwell on these points.

8. Another impediment is scepticism in the efficacy of medicine. Total scepticism is a heartless thing—an enemy to all enthusiasm, a drawback to all investigation. For if there be nothing to be learned, why should I yet seek to learn? It is a baneful error, arising sometimes from impatience of examination—often from a want of skill and ability. A practitioner once said, that every year served to diminish his faith in the efficacy of physic; and the remark was made upon having failed to procure alvine evacuations by large exhibitions of drastic purgatives. He forgot to attribute his failure to the right source—a neglect of blood-letting and other relaxing measures previously; for the case was attended by a strongly inflammatory condition. Whenever, indeed, we hear physicians frequently exclaiming against the efficacy of medicine, we deem it a presumptive argument of their want of skill. Their disappointments, most probably, are errores non artis sed artificum.

9. Another impediment, is the indifference of many to the acquisition of profound medical learning, arising from the conviction that such an endowment will be unappreciated. Mediocrity in physic too often supersedes higher merit. Professional men are apt to be slow in acknowledging superior excellence, and as to the people, he who can best humor their fancies is generally the best physician. Exhibitions of medical skill, from their very nature, are unappreciable by the uninitiated. Hence, old women, witches, and impostors, as is said by Lord Bacon, have ever been the rivals of physicians.

10. Another impediment, and we conclude our list. An inordinate love of gain, benumbing the faculties, or prompting them to just so much exertion as may be necessary for the acquisition of wealth;

tempting the practitioner to profit by more patients than he can possibly attend, and occupying all his hours with wishes, and schemes, and efforts for private aggrandizement—is a prevailing, serious impediment to the improvement of our profession. For what gifts can science expect from men solely intent on riches, who at the bed-side of a patient are more busied in anticipating the amount of their fees, than in estimating the peculiarities of the case?

We conclude by observing, that it would be wisdom for us to test fairly the virtues of original, reputable recipes—to put away from amongst us the disbelief in specific remedies—to avert or counteract the influence of absurd theories—to avoid hasty observations, and rash generalization—to suffer the influence of no eminent name to fetter or bias our inquiries—to promote improvement in the preparatory education of students, and reform in the examination of graduates—to take neither disheartening views of the difficulties, nor meagre views of the objects of medical science—to learn to distrust ourselves, rather than our art—to permit not the indifference of the world, or our professional brethren, to relax our efforts after excellence—and finally, to animate our bosoms with a fervent, disinterested desire for the attainable perfection of our art.

Miscellaneous Intelligence.

ANALECTIC, ANALYTICAL, AND ORIGINAL.

I. Analectic.

Ad Sylvas Nuncius.

1. *On Hysteralgia, or Irritable Uterus. By Dr. D. Davis.*—In that very excellent work now publishing in parts by Dr. Davis, we find an article on hysteralgia, which we deem worthy of notice. The term was first given to the complaint by the late Dr. Gooch, having previously gone under the name of "painful menstruation"—"uterine irritation"—"chronic inflammation of the uterus," &c. Dr. Gooch's description of the hysteralgia is adopted by our author, who next proceeds to criticise the theory of Dr. Gooch, who considered hysteralgia as a purely functional disorder, unconnected with inflammation, and not ending in any change of structure.

"This plausible theory, (says Dr. D.,) of an exquisitely painful disease, without the co-existence of inflammatory action of the affected organ, must at best, in the present state of our knowledge, be considered doubtful as to its correctness. It is not even certain that we are yet acquainted with all the possible forms of inflammation, so as to be competent to assert broadly and emphatically that this or that variety of inflammation should have a natural and necessary tendency to end in disorganization of structure. It is not easy to conceive of certain forms of rheumatalgic affections, for example, such as lumbago and sciatica, without connecting with them the idea of an inflammatory condition of the tissues principally concerned; and yet on that account, who ever supposes that such inflammatory actions have a natural and necessary tendency to end in malignant disorganization of structure? If muscular fibres be a constituent tissue of the uterus, why not such fibres become the subjects of a painful inflammatory affection, a truly rheumatalgic affection, without being followed, any more than in the other case, by a malignant disorganization of structure? It is well known that the uterus is not unfrequently the subject of very painful states, occasioned exclusively by functional causes, as we see constantly exemplified in cases of disordered menstruation, leucorrhœa, etc.; but does it necessarily follow, that such morbid con-

ditions are essentially independent of all inflammatory action? Or rather, is it not demonstrable that of some of them, at all events, inflammatory action is an essential attribute? And yet we find that such painful states, such demonstrably inflammatory affections, may be sustained for many years without producing malignant disorganization of structure. The limits subsisting between the phenomena respectively of irritation and inflammation, are not yet established with sufficient precision to enable us to determine with perfect confidence under which of these heads some doubtful forms of disease should be classed. Many diseases, loosely attributed to irritation alone, are often characterized by symptoms which a more accurate diagnosis would enable us at once to ascribe to actual inflammation. In the description of the irritable uterus as above quoted, we encounter several symptoms which are known to be constant accompaniments of inflammatory action. All the occasional causes of the disease, as enumerated by Dr. Gooch, as well as the greater number of its essential symptoms, would seem to lead to the supposition of a proximate state of parts, if not actually inflammatory, at least one of no inconsiderable vascular congestion; for in addition to a morbid state of the nerves of the affected organ, which is not disputed, there is also unquestionably a morbid over-dèstension of its blood-vessels during the presence of this disease: and this is, after all, the point of greatest importance practically to attend to, inasmuch as it bears immediately on the principal feature of the treatment to be adopted. The designation given by Dr. Gooch of 'the irritable uterus' to the distressing malady which he has here so admirably described, is therefore so far objectionable, as it leaves out of view one of its original, and perhaps its very principal constituent, viz. A PAINFUL OVER-PLENITUDE OF A PART AT LEAST OF THE INTERNAL ILIAC AND PUBIC SYSTEM OF BLOOD-VESSELS. Such a condition of the blood-vessels in question is more or less an obvious result of the occasional causes by which the disease is represented as being most frequently produced. It is promoted and exasperated by whatever exertions or other cuses which may be supposed calculated to increase the over-destension of the uterine vessels. The author recollects the case of a painful affection of the right foot, which was incurred by a gentleman some eighteen years ago by over-exertion in walking. At first the pain was considerable, and greatly interfered with the gentleman's pursuits; which required much personal activity. It was subject, like that of the irritable uterus, to occasional abatement, according to the degree of rest which could be afforded to the effected limb, and to more or less exacerbation, according to its exposure, which, indeed was unavoidable, to more or less of walking exercise. It was, however, at no time so severe as to render walking totally impracticable. For this reason the case was almost entirely neglected in the beginning. It consequently became a chronic affection, which, although it gradually abated of its original violence, has never altogether ceased to occasion inconvenience. Now the reader will easily recognise something of analogy between the occasional causes respectively of the irritable uterus and of the lamed foot. If the painful effects be not of

a nature to be identified with a state of inflammation in the one case, it would of course be quite proper to dispute its existence in the other. On the other hand, an over-extension of tissue in the one case might be expected to produce a similar result as to proximate effect to what is known to take place in the other. In the foot case a state of exhausted power was followed successively by an over-extension of fibres and a slow subacute inflammation of the injured tissues. Of the fact of the latter result, the author has most abundant reason to be quite certain. But why admit such results in the one case, and deny or totally overlook them in the other? Of the foot case the proper treatment undoubtedly would have been the application of a suitable number of leeches to the part, and the immersion of it for an hour or two afterwards in hot water, or the assiduous application for an equal length of time of hot fomentations to the surface, followed by a repetition of the same practice on the next, or on an early day, subsequently; giving also to the limb the benefit of two or three weeks' most perfect rest. But it may be very well asked, whether the idea of exclusive irritation could be supposed so directly to lead to the proper practice in such a case, as that of inflammation, or of that even of congestion of the vessels of the part consequent upon the application of the previous injury. The author thinks not. For the same reason he therefore thinks that the new designation of Dr. Gooch, as applied to the morbid condition of the uterus, which in many respects he most faithfully described, may have the effect of leading practitioners to an inert and procrastinating practice.—About two years ago a case occurred within the cognizance of the author, very well suited to illustrate the tendency of a name to impose upon a weak mind, in an affair precisely of the kind, or rather in the instance of the very disease which we are now describing. Mrs. S. of B. Crescent, a very delicate lady, of about thirty years of age, and the mother of a numerous young family, had been the subject of much uterine irritation for about eight months, for the relief of which nothing very efficient had been done by her ordinary medical attendant. The husband, without giving any intimation to that gentleman of his intention, requested the present reporter of the case to pay his lady a professional visit, and to favor him with his opinion of the nature of her malady, and of its probable issue. The neck of the uterus was found exceedingly painful and considerably swollen; but without structural disorganization. The patient was greatly attenuated, and very pale and spiritless. The case was reported as one of no urgent danger, but nevertheless one involving some ultimate risk, if the present symptoms, which were represented as those of a peculiar variety of inflammation could not be subdued. On being requested to see the patient again, he suggested the propriety of his being met by the family medical attendant. But that person was unappeasably offended at the husband for requesting another opinion without previously consulting him and without his consent, and declined all further attendance on the case. In a short time, however, afterwards, upon learning the author's opinion, he took great pains to represent it as being totally unfounded; adding, that if it should be acted upon, the practice would soon prove fatal

to the unhappy patient. The neck of the uterus, it has been already stated, was considerably swollen. With the aid of a speculum, it was seen to be also in a state of intense superficial inflammation. All its vaginal portion was of a vividly red color, similar to that of external genital surfaces when become the seat of a recent gonorrhœal affection. A quantity of viscid mucus was seen distilling from the uterine orifice. Little intimidated by the angry oracular prognostics of his predecessor, the author hesitated not to order four leeches to be forthwith applied to the orifice of the uterus. This duty was perfomed by the very intelligent midwife of the Maternity Charity, whose useful services in this respect he has already had occasion to notice. He was induced to limit the number of leeches to four, in consequence of observing how intensely the vaginal part of the organ was charged with blood. The quantity of blood obtained amounted to at least ten ounces, and the abstraction of it was almost immediately followed by the happiest results. After an attendance of about three months, during which the application of between four and six leeches were repeated four or five times, the author on retiring had the pleasure of leaving his fair patient in a state of much comparative comfort, of almost total freedom from the distressing pain of the uterus which had recently embittered her existence, and in other respects rapidly recovering her former health and strength. There was in this case very probably the irritability of the uterus, which had been represented by the family attendant as the patient's peculiar malady, but which had been in no degree mitigated by the soothing and strengthening medicines exhibited by him for its relief; but there was also most unquestionably much positive inflammation of the vaginal portion of that organ, the removal of which, by the depleting measure already described, made way for the eventual subduction also of the accompanying irritability. In the case of 'THE IRRITABLE UTERUS,' there is a period of recency and comparative acuteness of symptoms as certainly as there is in those of the irritable tumor of the breast, and of painful affections of knee and ankle-joints from over-extension of their ligaments; and there is little doubt but early and efficient vascular depletion would be quite as beneficial in all cases of the former, as they would probably prove in either of those of the latter. But would the hypothesis of the mere irritableness of the part in any one of these cases directly lead to such a practice? Again, the author thinks not. He accordingly finds local bleeding placed by Dr. Gooch under his second head of remedial measures; whereas the supposition of an over-fulness of the vascular system of the affected organ would naturally point to the relief of such a state AS A FIRST MEASURE. But if the disease be one of irritation and not of inflammation, nor of any condition of the parts allied to that of inflammation, why bleed at all? Because probably the utility of the practice had been fully ascertained by experience before the theory of the irritable uterus had presented itself to the mind of its talented propounder."

From the above observations it is evident that the author considers vascular depletion as forming the main feature of the treatment. But general blood-letting, he thinks, can rarely be necessary. The

disease is local, and local abstraction of blood from the os uteri, he avers to be the best remedy. The complaint is usually concealed for a long time, and consequently the cure is rendered thereby tedious. The application of four leeches to the os uteri will generally secure the abstraction of eight or ten ounces of blood, and be succeeded by relief of the symptoms. The second most important measure is the horizontal position—and these two means, if early had recourse to, would soon reduce the disease; but when become chronic, then the recovery is tedious.

"There is, however, one point of practice, in reference to this form of the disease, to which the reader will do well to pay particular attention. The subjects of "the irritable uterus," are not always unsusceptible of impregnation. On the event of conception taking place during a period of remission of its most urgent symptoms, the medical attendant should then more than ever, and especially during the earlier months of gestation, insist upon the strictest conformity to his precepts in respect to the observance exclusively of the horizontal position. The action of gestation introduces a great change into the uterine system. During the last four months it places the uterus in a situation to be in a great measure secure from the attacks, if not altogether beyond the reach of some of the most influential occasional causes, of the disease. On the completion of the process of parturition, the patient may indeed be said, in reference to her former complaint, to have the opportunity of commencing a new life. If, during that period, she could be induced to keep her bed, in the most literal sense of that expression, for six weeks or two months, she would almost certainly secure herself against a relapse of her complaint subsequently to her confinement. In consequence of the prodigious development of parts interested in the business of gestation, nature is observed to exhibit a power of self-restoration and adjustment during the puerperal states, which at no other time nor under any other circumstances does she seem competent to exert. Hence, in cases of moderate prolapsion of the uterus incurred by forward conduct, during one confinement a perfect cure may frequently be obtained by the patient confining herself to her bed, and maintaining rigidly the horizontal position for at least five or six weeks subsequently to her next delivery."

Anodynes are necessary, and opium is the most efficient; but as it too often confines the bowels, and checks the biliary secretion, hemlock, hyosciamus, &c. combined with camphor, become necessary as substitutes. Battley's laudanum thrown into the rectum will often succeed, where it is found to disagree if taken into the stomach. As an aperient, Dr. Davis recommends sulph. magnes. in infus. rosar.—castor oil—electuary of senna—sulphur. In some cases of irritable uterus, accompanied by obesity, Dr. D. has seen good effects from mercury, as an alterative.—*Medico-Chirurgical Review.*

2. *Discharge of Worms from various parts of the Body.**—John Alexander, aged 10 years, was for nearly a year in a delicate state of health; although his appetite continued pretty good, he had much wasting of flesh and general debility. About eight months since a tumor arose over the epigastrium, which after being poulticed, for some days, burst and discharged, with about two ounces of pus, a white worm half an inch long. In a few days the abcess healed. Eight or ten days afterwards a second tumor arose about three inchess distant from the first, on the right side of the chest, which after some days also burst and gave exit to another worm. It is needless to particularize the different instances, suffice it to say, from the time of the first worm being discharged until I first saw him, which was an interval of two months, five worms had made their appearance. They were all similar to the first, and lived for a few hours after their discharge. When I saw him the integuments of the right cheek and eye were excessively swollen, and in the course of a few days another worm was discharged from the upper eyelid. I recommended different medicines for the space of six weeks, but the formation of abscesses on different parts of the trunk and extremities proceeded, and altogether about twenty worms were discharged, principally from the right side. At length two grains of calomel were given every night until the gums became affected, and convalescence shortly afterwards took place, I shall not pretend to say whether from mercurial influence, or from the produce of the original nidus having been exhausted. The boy has been for now nearly three months quite well, his health and strength being completely re-established. The worms appeared to be ascarides. None were, however, at any time observed to be discharged from the intestines, nor were the bowels irregularly affected.

I cannot account for their formation: whether the first deposition of eggs had been made by some means under the external skin, or whether a worm had perforated the intestine, and at length made its way to the surface. I incline to the latter opinion from the boy's previous ill-health. I could in a few instances trace a reddish line from one abscess to the other, this was, however, after the new abscess appeared; the patient himself felt no uneasiness in the part, nor had he any idea where the next abscess would form until it appeared externally.—*Ibid.*

3. *Mr. Robertson on Dry Cupping.*†—Mr. Robertson, an intelligent surgeon residing in High Holborn, has published some interesting cases of pain depending on various causes relieved by dry cupping. The cases in which he would recommend it are those in which the pain is dull though severe, deep-seated, chronic, not much increased by pressure, or has refused to yield to ordinary means. The way in which he generally employs the remedy is to throw a very minute bit of paper touched with ether or turpentine, lighted, into a large glass

* Extract of a letter from Mr. C. Neilson of Killala.

† Lancet, January 19th, 1833.

or tumbler, and press it down in the usual way. This is a very effectual and a very convenient method of obtaining the requisite exhaustion, and is attainable when the regular cupping apparatus is not. We will select two cases out of eight related by Mr. Robertson. They are the most interesting and perhaps the most satisfactory.

Case 1. *Spasmodic Pain in the Loins, post Coitum.*—"A stout young gentlmen, and in fine health, was seized shortly after connexion, with most violent deep-seated pain in the region of the *left* kidney; so severe that he was unable to walk, stand, or sit. Lying on his back on bed, or on a sofa, gave a little, and only a little, relief, and frequently did not relieve him at all. He had been often attacked similarly before, and traced it distinctly to connexion. The peculiar sensation in the part commenced immediately after coitus, and could be felt distinctly increasing more and more till it ended in a paroxysm. Sometimes the sensation went off altogether, particularly if he carefully kept the recumbent position; but when it did not, four or five hours would intervene before the paroxysm came to its height. In one instance he was attacked severely while walking home, and had very nearly fallen down in the street. In another he was awoke in the night. Twenty or thirty leeches relieved it the first time. In about two months it came on again, when bleeding at the arm and 70 leeches over the seat of pain, gave him only moderate ease.

After some month's interval it returned a third time, and leeching, even to a very great extent, seemed to have altogether lost its power. A blister and some internal medicines were prescribed by a physician with little relief, when I ordered 20 leeches to the anus; this, and the recumbent position for two days subdued it.

A fourth time he felt it coming on. This was at midnight, (connexion having taken place some four hours previously,) and when I saw him, the pain in the left lumber region seemed frightful, deep-seated, and of that peculiar nature, that he could bear almost any pressure on the part without shrinking. He lay in bed writhing like a serpent.

I felt loath to bleed him to such an extent as had been requisite formerly to subdue it. Leeches were not at this time conveniently to be had, and I determined to try the effect of dry cupping A very large tumbler was put over the part, kept on for a minute or two, till it seemed to gall him; taken off, and replaced three or four times. The effect delighted and almost astonished me. One minute after it went on, the most perfect relief was felt; the pain was entirely gone; so afraid was he of its return, and so keen to have the glass on, that he insisted on having it on and on, till the edges of the tumbler had almost cut into the muscles! This he declared he cared not for. It was a trivial thing compared to the dreadful and insufferable pain in his side. I knew him for years afterwards, and though the cause was continued as before, he never had any return."

Mr. Robertson susposes, reasonably enough, that the pain in this instance depended on spasm of one of the lumbar muscels.

Case 2. *Pain in the Left Umbilical Region.*—"A very interesting young lady, the wife of a friend of my own, left —— for Edinburgh, immediately after the marriage ceremony was performed; and when about half way, was seized while in the carriage, with most violent pain in the left umbilical region. Her husband managed to get her by easy journeys to Edinburgh, where she remained about three weeks, and was bled, blistered, purged, and put through all the ramifications of the strictest antiphlogistic system.

At length she was obliged to be brought home by short stages, a distance of 40 or 50 miles; and was ultimately relieved by turpentine enemata, which brought away some discolored hardened feces. Her menstrual periods had for years been attended with extreme pain. During the two years which followed marriage, she was said to have had two miscarriages. During the third she miscarried again, and was getting round, when I was suddenly sent for, on account of an alarming pain *in the very spot which had formerly been so productive of suffering*. She could not account for it; she had been lying quietly in bed, and had been eating and drinking nothing to produce it. The lochial discharge went on as usual, and her bowels were natural, as she had, ever since her former attack, used Maw's instrument. Leeching was proposed, but to this I objected, on account of the loss of blood she had sustained so lately, and from the effects of which she had not as yet recovered.

I determined at first to try dry cupping; she ascented, and as the things are always at hand, in five minutes she was so well as to be able to joke with her husband and me about 'the very troublesome wife' the former had. It never returned."—*Ib.*

4. *Use of Turpentine in Sciatic Neuralgias.*—M. Martinet has adduced a long catalogue of cases to show the superior efficacy of small doses of the oil of turpentine. To prevent its acrid effects on the stomach and bowels, he recommends that it be always blended with some corrective excipient, such as honey, gum-arabic, magnesia, yolk of egg, &c. and the dose ordered is a drachm or two in divided doses, daily. As a matter of course a correct diagnosis must have been previously made, in order that we may be satisfied that there is no inflammation or organic disease of the nerves; under such circumstances we cannot reasonably expect a cure from the turpentine alone. If the drug vomits or purges to excess, opium should be added. In 40 cases of acute neuralgia, 34 were cured, 5 relieved, and 1 was not benefitted. In 31 chronic cases, 24 were cured, 3 were relieved, and 4 experienced no advantage: 33 of the cases treated with the turpentine had resisted other remedies previously employed. The period generally required for the cure was from 5 to 12 days; in a few cases the medicine must be continued longer. Out of 58 cases 48 were sciatic, 3 crural, 4 brachial, and 3 facial neuralgias.—*Bullet. de Therap. from Medico-Chirurgical Review.*

5. *Lisfranc's Treatment of Amaurosis.*—First of all, we should ascertain whether there are any symptoms of inflammatory fulness and

activity in the eye or head;—as a matter of course, such cases require depletion; when, however, we have reason to believe that the disease is one rather of debility, Lisfranc strongly advises us to direct our attention in an especial manner, to stimulate the frontal and other branches of the fifth pair of nevers by means of repeated blistering over the eyebrows and temples. Should this fail, we must endeavor to excite the torpid organ by acting immediately on the ciliary nerves, any irritation of which is speedily propagated to the ophthalmic ganglion and the origin of the trigeminus. This is most effectually done by the application of stimulants to the cornea; and of these stimulants the nitrate of silver in substance is the best. The inferior segment of the cornea is to be lightly touched, till we perceive a whitish cloud;—the eye is then to be immediately washed with water. Considerable pain is felt; the whole apparatus of the eye is put into a state of so increased activity, that on the morrow a stranger might suppose that our patient labored under acute ophthalmia. This treatment induces sometimes vomiting; and as it always occasions temporary contraction of the pupil, it must not be employed when there is a tendency to this evil. The operation requires to be repeated several times.—*Journ. Complem.—Ib.*

6. *On Hydatids, and their Conversion into Tubercles.*—M. Kuhn has lately read before the French Academy a memoir on acephalocysts, and the manner in which these parasitical productions give rise to tubercles. He holds the opinion of Laennec, Bremser, and others, that they are to be considered as truly of an animal nature; and draws a distinction between those found in the human body, from what are often seen in sheep and other lower animals; the former, says he, are always propagated by internal buds, or growths which are thrown off from the inner surface of the original hydatid, and may be, therefore, denominated "endogenous;" they may be compared to a nest of boxes, one within the other, whereas the latter produce only on their outer surface, and are, therefore, "exogenous." It sometimes alone, sometimes simultaneously was after a very careful examination of the lungs of oxen, which had died of a species of phthisis called "pommeliere," that M. Kuhn was led to the belief of the degeneration or conversion of hydatids into tubercles. The hydatids, by their irritation, causes cysts to be formed around them; these cysts become stronger, fibrous, or even cartilaginous; meanwhile, the acephalocysts enlarge by serous imbibition, and multiply by buds from their inner surface; these again, in course of time, give rise to others, the whole nest being contained in one bag. From the inside of this bag is secreted a yellowish viscid matter, which becomes thicker and thicker; M. Kuhn regards it as the primitive tuberculous deposit; it gradually solidifies, and, with a simultaneous shrinking of the cyst, tends to squeeze and kill the enclosed animals, thus giving rise to a nucleus of tubercles. Sometimes the tubercles are not entirely filled up, but are hollow, and we observe only the shell or dried husk of the acephalocyst; we may even separate the thin layer of the animal from the debris within, by im-

mersing some of the tubercles in water. M. Kuhn has enriched his memoir with beautiful illustrative drawings; they throw much light on the etiology of the tubercles which are found in the lungs and liver of ruminating animals. The co-existence of hydatids and tubercles, in the same organs, is a fact at once curious and most interesting. The subject is one of much importance, and deserves future examination.—*Revue Medicale.—Ib.*

7. *Chloride of Lime as a Lotion, against the Itch.*—Professor Fantonetti gives the result of nine trials. In seven of these cases the disease was cured in from six to eight days; in the 8th case the psorous eruption was removed, but was followed by an eczema, which gave way under the use of tepid baths; and he left the hospital well; the itch however reappeared, and then yielded, only to sulphureous fumigations. In the 9th, the psora was cured—re-appeared—was cured again—returned a third time—and was finally subdued by the sulphureous fumigations. The chloride of lime lotion is made, by adding 1½ or 2 ounces of the chloride to a pint of water; it ought to be rubbed three or four times a day on the affected parts. A simple tepid bath is to be used every third day, in order to soften the skin, and clear it of any calcareous crusts. If the lotion is too irritating, its strength must be reduced. The disease is generally cured effectually in eight days.—*Annali Universali.—Ib.*

8. *Dupuytren's Mode of Treating Prolapsus of the Rectum.*—This eminent surgeon has cured a vast number of patients laboring under this disease by means of an operation which is both simple and speedily effectual. He seizes the folds around the anus with forceps whose blades are large and flat, and excises them with strong sharp scissors; this excision must be carrid deep enough to remove not only the integuments embraced by the forceps, but a portion of the rectum, when the relaxation of its mucous coat is very considerable; generally the depth of the wound need not exceed a few lines, but in other cases it must be at least an inch. Dupuytren usually cut off in this manner four folds of the margin of the anus, one in front, one behind, and then one on each side; if the disease be not of great extent, the removal of one or two of the folds may be sufficient. This operation is seldom followed by any considerable hæmorrhage. It is necessary that the patient be so treated, that he has no occasion to have the bowels relieved for eight or nine days, in order that the wounds may not be disturbed. The cure is generally complete in a fortnight. Dupuytren has met with no case of failure hitherto.—*Journal Complementaire.—Ib.*

9. *Discovery of a New Principal in the Serum of the Human Blood.*—M. Felix Boudet, in a memoir presented to the Institute, has investigated anew the composition of the serum of the human blood. His researches prove, that when evaported to dryness, exhausted by boiling water, and dried anew, it yields to boiling alcohol the following substances; 1*st*, a peculiar immediate principle, called by the author

seroline; 2*d*, *cholesterine;* 3*d*, a *soap*, soluble in water, most probably formed by the margalate and oleate of soda; and 4*th*, the *fatty matter of the brain.*

Seroline in deposited, on cooling, from the boiling alcohol by which the serum has been exhausted. The liquor filtered, when cold and evaporated, leaves a residue of a resinous consistence (*de consistence terebinthineuse.*) On treating this with clod alcohol, at 36° (*specific gravity* 0.8252,) the fatty matter of the brain is separated, and a substance is dissolved, which is considered by M. Lacanu to be an oil, but proved by M. Boudet to be composed of several distinct principles. In fact, the liquor left to itself, deposits crystals of cholesterine, and retains the soap mentioned above, together with a little of the fatty substance of the brain.

Seroline is white, of a slight pearly-color, in the form of filaments, which, viewed by the microscope, present the appearance of globules, or globular swellings. It melts into a colorless oil at the temperature of +36° (97.8. *Fahr.*) It has no action on colored re-agents, but like cholesterine, becomes red on the contact of concentrated sulphuric acid. Sulphuric ether dissolves it easily; alcohol on the contrary, at 36° (*sp. gr.* 0.8252,) dissolves only a trace of it at the temperature of ebullition, but has no effect on it when employed cold. Treated hot for six hours with *aqua potassæ*, it seemed not to experience any modification, and hydrochloric acid produced to the least disturbance in the alkeline liquor. Neither acetic or hydrochloric acid produced any apparent alteration on it, whether hot or cold. When heated a long time with nitrih acid, it was not dissolved, but became soluble in *aqua potassæ*, to which it communicated a brown color. Distilled by the lamp in a small glass tube, it diffused a very characteristic odour, furnished some alkaline vapors, a trifling residue resembling charcoal, and appeared to be in part volatilized. The small quantity of *seroline* obtained, did not permit M. Boudet to submit this substance to a greater number of tests, but he conceives that enough has been done to prove it to be a new principle.—*Ed. Med. and Surg. Journ. from Journ. de Pharmacie, June*, 1833. *from American Journal.*

10. *Pathology and Treatment of Gastritis.*—Extracts from a clinical lecture by Dr. William Stokes.—Numerous and important advances have been made in modern times, in the knowledge and treatment of gastric affections, and an enormous mass of facts have been accumulated of the deepest interest to practical medicine. It is, however, an unfortunate circumstance, and calculated to excite much surprise, that a knowelge of the various forms of this inflammation is not sufficiently spread among British practitioners. There is still a great deal of ignorance and misconception on this subject; many persons are still accustomed to take a limited and superficial view of it, and a thorough acquaintance with the various modifications of gastric disease is at the present day any thing but general. At one time we hear it called disease of the liver, at another time dyspepsia, sometimes it is termed constipation, and sometimes derangement of the digestive organs. It is true, that in such cases we find constipation, dyspep-

sia, and derangement of the digestive organs, yet these terms, as they are commonly employed to designate the disease, are at once both useless and improper, because they convey no correct or pathological idea. We owe, I think, to Broussais a great deal of our knowledge of gastric and enteric inflammation; it was his researches that gave the first clear and luminous view of a class of diseases previously obscure and little understood. He failed, however, in procuring the general assent of the profession to the propositions he advanced, and one of the principal reasons of this failure, and of the partial diffusion of the knowledge of gastritis in this country is, that although he brought forward a great number of valuable facts, he also promulgated a theory which has not been cleary or successfully proved. This theory has been rejected, and with the theory British practitioners rejected his facts. * * *

Gentlemen, we have had no acute cases of gastric inflammation of late in our hospital wards, but there are two cases of chronic affection of the stomach, on which I purpose to offer a few remarks. The first is the case of a man in the chronic medical ward. This man is from the country, and is at present laboring under an affection of the stomach, exceedingly common among the Irish peasantry. I have seen a great deal of it in the course of my own practice, and any person residing in the country, if he happens to be a medical practitioner, must have been repeatedly called on to treat this form of disease. At least persons, who labor under it, are chiefly of that class who live a good deal on potatoes, and I am inclined to think this exclusive diet must have some share in its production. Most of those, who come into hospital with this complaint, are from the country, particularly persons whose circumstances have been impoverished, and who consequently have been compelled to change their former nutritious and better food for a potato diet. In such cases, we generally find those persons able to date the commencement of their illness from a poriod (immediately) subsequent to this change, and it is therefore not improbable that the change of diet has some influence in the production of this peculiar gastric affection.

The patient, who is the subject of the case before us, is somewhat reduced in flesh and of a sallow complexion. He has complained of pain in the region of the stomach, extending to the back, right hypochondrium, and shoulder. He has had tenderness over the epigastrium, loss of appetite, pain and sense of distention, increased after eating; vomiting of yellow matter occurs two or three hours after taking food, succeeded by thirst. His pulse is soft and slow; tongue clear; bowels open. His illness, (and this, I think, is a point worthy of remark,) commenced four years ago with pain in the stomach, increased by eating and relieved by vomiting; and some time after this the vomiting began to be succeeded by thrist. The vomiting generally came on in an hour or two after taking his meals, and he threw up a quantity of yellow slime. Another important point is connected with the treatment, he has undergone for this disease. He has been relieved by antiphlogistic treatment, locally employed; we treated him since his admission by leeching, blisters, and cupping over the stomach, and

latterly he has been using narcotics. Ninety-nine persons in a hundred would be inclined to call this a case of dyspepsia, and so it is so far as loss of appetite and derangement of the stomach are concerned, but the worst of the thing is that they would look at it only in a single point of view, and treat it as mere nervous disease of the stomach. Well, with respect to this man's case, it is either a chronic gastritis, or it is a nervous affection, and mere dyspepsia. Before we proceed further, let us see what is the precise meaning of these terms. Dyspepsia is a name given to that condition of the stomach, in which, without any sensible alteration of structure or circulation, the stomach does not perform its functions in a regular and proper manner, but there is no organic lesion; and if a man, laboring under simple dyspepsia, were to die, we could not detect any change in his stomach, so far as circulation and structure are concerned. Chronic gastritis is a lesion of the stomach, with a change of its circulation and a thickening of its lining membrane, or, in orther words, with signs of actual disease in the stomach. Now, in my opinion, there is a great probability that this affection, which is so frequently observed among the peasantry of this country, is a chronic gastritis. It may, I grant, commence by dyspepsia, but, in its advanced stage, and it is only in this stage that such cases come under the care of medical men, it is most commonly a chronic gastritis. We have heard, it is true, no post mortem examinations of this disease, and can, therefore, only reason on probabilities, but if we look to that form of treatment which has been found most successful in affording relief, we find it to be that which is calculated to remove irritation and vascular excitement. Besides, the antidyspeptic treatment has failed, after extensive and repeated trials. In fever this gastritis is of very common occurrence; and here also it is most advantageously met by local antiphlogistic means. But there is another circumstance which you should always keep in mind: by the time you get persons laboring under this gastric affection to come and place themselves under your care, they have been ill for months, and pephaps years. The disease is certainly at this time very chronic; and you are aware that it is a general pathological law, of the truth of which we have the most ample proof, that where an organ has been long subject to functional derangement, there is a strong probability that more or less of organic change has also taken place. We seldom see the brain, or the lungs, or the kidneys deranged in function for many years without more or less of structural alteration; and we may conclude that any viscus, in which functional disorder has existed for a considerable period, will ultimately experience organic change. If, then, we connect with these facts the failure of the antidyspeptic plan and other circumstances, we are led to infer that these and similar affections are cases of chronic gastritis. I do not say it is so exactly, but I think the collateral proofs are very strong in favor of its existence.

Well, what have we done in the present instance?—We have endeavoured to regulate the man's diet; we have cupped and leeched the epigastrium, and afterwards employed counter-irritation. In all cases of a similar nature our treatment has been nearly the same; in

most it has been followed by permanent relief; but where this did not occur, and the patient complained afterwards, we have had recourse to narcotics. This man has been laterally taking, with the most signal benefit, the eighth part of a grain of acetate of morphia twice a day. You will see in Dr. Bardsley's Hospital Facts and Observations that the acetate of morphia has been employed with singularly good effects in the treatment of gastric affections; and where its use has been preceded by leeching I have a full conviction of its value, as well as that of various other narcotics.

There is, gentlemen, another case,—that of the patient Denham, who has been complaining of pain and tenderness in the epigastrium, with loss of appetite, and intolerable thirst. His face and extremities are œdematous, urine not albuminous, bowels confined. His tongue is red, and thickly coated with fur; his illness commenced two months since. I looked on this as a case of chronic gastritis; for, you observe, he had all the symptoms, pain, tenderness of the epigastrium, red tongue, impaired appetite, and an insatiable desire of cold drinks. We treated him by leeching and blistering the epigastrium; we gave no purgative by the mouth, but obviated the costiveness by enemata. By this treatment much good has been effected. Since the leeching and blistering, his thirst, which was so excessive that I thought at one time he had diabetes, has completely declined; his tongue is much improved, and he no longer complains of any gastric pain. His appetite, however, continues bad; and it will remain to be seen by the progress of the case whether this depends on want of tone in the stomach or actual disease.

In submitting these cases to your notice, it may not, perhaps, be irrelevant to make some observations on the pathology of gastritis in general. Many circumstances tend to prove that chronic gastritis is a very common disease. Although not admitting of direct proof, I think it is also true, that where dyspepsia has lasted for a long time, there is more or less of gastric inflammation. Nothing is more common than dyspepsia; and hence, in all probability, chronic gastritis is common also. We are not, however, to go the whole length with Broussais, and give the name of chronic gastritis to every case of dyspepsia which comes before us. Broussais is too much of a solidist,—he refers too much to the mere visible alteration of organs, and his idea is, that every case of dyspepsia is a case of gastritis; that there is scarcely such a thing as mere nervous dyspepsia; that all instances of this kind are only various forms of gastric inflammation, and to be treated as such. Here it is evident that theory has led him astray; for that this notion is incorrect has been proved by the circumstance that several cases of dyspepia have been relieved by treatment not calculated to remove inflammation. We every day see cases of dyspepsia deriving the most decided benefit from the use of stimulants, wine, and a generous diet; and where this occurs, who is there that would venture to call them chronic gastritis? But although we do not go the whole length with Broussais, and justly reject the speculative part of his doctrines, still we owe a great deal to his industry and research: he has brought forward a multitude of valuable facts which were

formerly but little appreciated or understood; and when you are called on to treat a case of dyspepsia, I think you should take the greatest pains to ascertain whether it be chronic gastritis or not.

The next thing I have to remark is, that it is extremely difficult to make out the diagnosis of chronic gastritis; we do not know one symptom which would enable us to draw a line of distinction between dyspepsia and chronic gastritis. You will read in books a minute detail of the symptoms by which they are separately characterized, and will think yourself capable of making a diagnosis; but when you come to practice, even in an hospital, you will find the affair involved in very great perplexity. Another thing is, you will have all the symptoms and causes equally prominent in the early stage of both. I have often stated, that if I were examined on this subject, and required to give a diagnosis between gastritis and dyspepsia, I could not tell the diagnosis. The truth is, their symptoms are identical. In chronic gastritis there is no fever, and the pain, flatulence, distention, acidity, loss of appetite, &c. are the same in both. I feel convinced that chronic gastritis is very often confounded with dyspepsia by British practitioners. It is treated as disease of the liver by blue pill and black draught; it is treated as dyspepsia by tonics and stimulants; it is treated as constipation by drastic purgatives. Constipation arises from a variety of causes, frequently from inflammation of the upper part of the tube. Now, observe the result of mistaking chronic gastritis for any of these three complaints. If it be taken for dyspepsia, it must certainly be increased by the tonics and stimulants which form the great bulk of antidyspeptic remedies. Run over the whole class of antidyspeptic remedies, and you will find them to consist chiefly of powerful stimulants. If it be treated as disease of the liver, of course blue pill is given, but what is the consequence? The liver is stimulated, and there are copious bilious discharges; but the true cause of the disease, the gastritis, is wholly neglected, and, by neglect, becomes certainly worse. It will be also neglected, and even much exacerbated, if taken for constipation and treated as such. How are you to make the distinction and steer clear of error where your course is obstucted by so many difficulties? Recollect the rules which I have before given on this subject. If the disease is chronic, the probability is, there is more or less gastritis in it, and the more chronic it is the stronger is that probability. In the next place, when patients apply to you for advice they are generally a long time ill, and have gone through several courses of antidyspeptic remedies. Now, if you happen to get a patient who has been treated for months, or even years, with blue pill, bitter tonics, and stimulants, and find that he is rather worse than better, you have two data to go upon; your diagnosis will therefore be more likely to be formed safely and accurately, and your treatment successful.

You will ask me, perhaps, would I never employ tonics in the early treatment of dyspepsia? To this I will answer—never, in case it should be combined with gastritis. Here, however, I must remark, that Broussais has gone too far in restricting such cases to a pure antiphlogistic treatment throughout, for I believe there is a period when

such treatment will do no good. When we have completely removed all irritation by the former plan, I think we may then have recourse to tonics with decided benefit. When we consider the curative action of tonics, stimulants, and bitter medicines, in the treatment of the majority of diseases where they are employed, we find that they are most efficacious and successful when preceded by a judicious antiphlogistic treatment. We shall see more of this as we proceed.

There is another case which I wish to notice: it has been, I believe, one of an acute character; I allude to that of the man in the Fever Ward. This person, after committing an excess in drinking, got sickness of stomach and vomiting. In your investigations of any case which comes before you, it is of importance, towards finding a cerrect diagnosis, to hold these two things in view—the exciting cause, and the first symptom of disease. Here you have, in the first place, excitement of the stomach from the use of spirits, and aftewards irritation, manifested by the vomiting. This was followed by loss of appetite, constipation, pain in the lower part of the left hypochondrium, foul tongue red at the tip, symptoms which indicate irritation of the mucous membrane of the stomach and intestines. When he was admitted into the hospital, however, what he chiefly complained of, and what were certainly the most prominent symptoms, were tightness across the chest, great difficulty of breathing, and harassing cough. His cough was indeed very severe, his sputa slightly tinged with blood, his breathing very much accelerated, and, to a superficial observer, he would appear to labor under chest disease. But, remark, we found not that he had been complaining of these symptoms for about three weeks, and consequently, if they had been pulmonary symptoms, they must have proceeded to a very alarming extent in that time. Mr. Lees examined him by percussion and with the stethescope, but could not detect any disease of the lungs, and he was examined by myself on the next day with the same result. Moreover, the patient had been previously treated for pulmonary disease without success. We were, therefore, led to conclude that there was no original disease of the lung, but only sympathetic irritation, depending on gastritis. We took small quantities of blood from his arm, leeched the epigastrium, kept his bowels open by enemata, and under this treatment we saw all his symptoms disappear, as it were, by magic. This is a remarkable case, giving us an illustration of the manner in which gastritis may simulate other diseases, and exhibiting the importance of attentending to the exciting cause and first symptoms of a disease in order to arrive at a correct diagnosis. Here you see it putting on the semblance of pneumonia, in other cases it assumes the guise of encephalities. It was remarkable, that the bleeding and leeching increased rather than diminished this man's strength, for after their employment his prostration nearly disappeared. You will see a great many of these cases in the course of practice, where the primary mischief is masked by a train of prominent sympathetic phenomena, and in which your diagnosis is to be founded on the following circumstances. These persons labor under a kind of fever; there is generally severe harrassing cough, and respiration is considerably accelerated; their

symptoms have been of several days' duration, and when you examine them with the stethescope, you find that the pulmonary symptoms are not accompanied by corresponding organic lesions, and have not advanced in proportion to their duration. You either find no disease at all in the lungs, or a bronchitis too slight and trivial to account for such alarming symptoms. You next examine the larynx, and finding there no evidence of morbid change, you look for the cause of the cough in the digestive tube, and most commonly trace its existence to a concealed gastritis. If you meet with a case in which violent cough, labored respiration, and other symptoms of pulmonary disease have existed for a considerable time, without any signs of disease of the lung sufficient to account for them, you may often sit it down as a stomach affection, and direct your treatment acccordingly. Generally speaking, this simulated phenomena is most commonly met with in children, but instances of it in adults are by no means rare. Another thing is, if you happen to have tried pectoral medicines, and found them to fail, your diagnosis will be more certain. You remarked the case of a man who was here some time back, laboring under what may be called a tussis firma; well, this was dependent on gastritis. We treated him with leeches to the epigastrium and iced water with most signal benefit. He committed some excess in eating, and had a return of his complaint; he was treated again in the same way, and recovered. I remember having attended a lady some years ago, who complained of some feverishness, with very severe and harassing cough. Not being aware of the nature of the disease, I treated it as a case of fever, with irritation of the bronchi. The fever declined, but the cough continued without amendment; I was much embarrassed by its obstinacy, when one day, happening to be in attendance, the lady remarked that she had been under Dr. Cheyne's care for a similar complaint, and drived much benefit from leeches to the epigastrium. On this hint I acted; the leeches were applied immediately, and my patient's cough entirely disappeared. You will observe this is the point to which I would direct your attention; consider that the diagnosis depends on the persistence of pectoral symptoms; consider that if it were disease of the lungs, it would, in the course of two or three days, produce lesions capable of being easily perceived. But if this be the case, and you look in vain for any organic change to account for such excessive cough, you will seek for its cause elsewhere, and refer it to sympathetic irritation, produced by disease of some other organ, and this is most commonly the stomach. In connexion with this, I have to notice a very interesting fact in the pathology of gastritis. In such cases as the above, you will generally find but little direct evidence of gastric irritation. The patient has no vomiting, he complains of very little pain, and the epigastric tenderness is very slight. Here is the law by which such affections are regulated. In those cases in which the sympathetic irritation is most strongly marked, the usual or local symptoms of the disease are least apparent. We see cases of this kind apparently consisting of chest disease, and sometimes even assuming the appearance of cerebral disease, or tetanic symptoms, while the true signs are completely masked. You will find

in Andral's work, a remarkable case of this kind, in which the ordinary symptoms of fever, vomiting, pain, epigastric tenderness, continued for a few days, when tetanic symptoms set in, and immediately those which were indicative of gastric irritation disappeared. But we are not to be deceived by the supervention of sympathetic irritation manifesting itself in other organs, nor are we to suppose that the gastric affection has subsided because we have an imposing train of symptoms existing in other parts. As long as the irritation continues, no matter in what organ it appears, we have strong evidence that the disease, though lurking, is still unsubdued. It is of importance to bear this in mind when you come to treat cases of sympathetic irritation depending on gastritis. If you treat them as pulmonary disease, as any superficial observer, or any person unacquainted with the use of the stethescope would be liable to do, your mistake will be, indeed, a very serious one. In the first place, the gastritis will inevitably be increased by being neglected. In the next place, though the internal remedies which are ordinarily employed for the removal of pulmonary affections, as tartarised antimony, squill, and other similar means, obviously produce the worst effects in gastric disease, and must tend materially to its exacerbation, so that there are in such instances two sources of exasperation, one arising from neglect, the other from the employment of therapeutic means which are totally contra-indicated by the nature of the disease. Remember, therefore, that where there are violent symptoms of disease of the lungs, and where these have gone on for several days without any porportionate lesion of these organs, that you may look for their cause and origin in a concealed gastritis. Recollect also, that in such cases the gastritis may be nearly latent, and want most of those symptoms by which it is generally characterized.—*Lond. Med. and Surg. Journ. May 25th*, 1833, *from Am. Journal.*

11. *Inflation of the Bowels.*—Mr. Blacklock, in a communication in our cotemporary, the *Glasgow Medical Journal*, for May, 1831, states that about thirteen years previously he attended a child whose bowels could not be moved by any of the agents usually employed in cases of obstruction, although there were no symptoms of inflammation till towards the close of the scene. Having obtained leave to examine the body, nothing could be discovered to account for death, but a very complete intussusception in the course of the ileum, and which immediately disappeared on inflating the bowels with the blow-pipe. It occurred then to Mr. B. that this might have happened had the bowels been inflated during life. Mr. B.'s son, about three years of age, was attacked with constipation, so that during nine days no evacuation could be procured from his bowels, notwithstanding the most persevering administration of purgatives, and the almost hourly use of enemata. At length Mr. B. had recourse to inflation, and the child immediately had a free evacuation. Mr. B. has since tried the remedy frequently, and often with the best result.—*Ib.*

12. *Iodine in the Treatment of Salivation.*—In a recent number of

Hufeland's Bibliothek der Practischen Heilkunde, there is a note strongly recommending iodine in cases of severe salivation, which is represented as removing the most violent inflammation of the salivary glands, and even healing ulcerations produced by mercury within a few days. The dose is two grains a day, increased to four. The following is the formula: ℞. Iodine, gr. v. solve in spt. vin. rect. ʒij.; adde aq. cinnam. ʒijss.; syrup. simp. ʒss. Dose, half a table-spoonful, and gradually increased.—*Med. Gaz.—Ib.*

13. *Inflammation of the Mucous Membrane of the Bowels.*—Extracted from the Clinical Lectures delivered at the Meath Hospital. By Dr. William Stokes.—This case would appear at first sight somewhat perplexing; but, by considering that this patient has had no irritability of stomach or vomiting, that during the course of his disease he has had thirst, but no desire for cold drinks, and that symptoms of irritation of the lower part of the bowels have been absent, you will be able to infer that it must be inflammation of the intermediate part of the digestive tube. This patient is, however, still in a precarious state, though he derived much benefit from the application of leeches to the right iliac region, and his head-ache rapidly subsided after their use; his tongue is still foul, and he continues very feverish. On Saturday he was very ill; he complained of ardent thirst, his respiration was fifty in a minute, but the stethescopic signs of disease were insufficient to account for such acceleration of breathing. Now, you all will recollect, that I have often told you that where there is fever and extremely hurried respiration, without any distinct evidence of disease of the lungs or windpipe, we should always look for the source of the disease in the digestive tube, and that this is most commonly found to reside in the stomach. In this man's case we could not, by the stethescope or percussion find any cause for the increased rapidity of respiration; but we observed that his belly was swollen and his thirst urgent. We applied the leeches again on yesterday with the most extraordinary benefit; the head-ache, tympanitis, and labored respiration were manifestly relieved. An objection might be raised to the efficacy of this mode of treatment, as the improvement took place on the fourteenth day, and it might be said that it was an improvement which depended on a crisis. To this I will answer, that I have seen so many cases of improvement after leeching without crisis, that it is unnecessary to take this into consideration, and that in the present instance there has been no crisis is obvious, as the patient is still in a bad condition. If all his symptoms began to decline on that day, then indeed the effect of crisis might be reasonably inferred, but his original affection still continues; and therefore it is fair to conclude that his improvement is attributable to the remedies employed. There is another point with respect to leeching in gastro-enteritic fever; *we have seen numerous instances of crisis brought on by the application of leeches to the abdomen.* This is a curious circumstance; but I have seen it occur in so many cases that I feel convinced it would not have come on if the leeches had not been applied. I have seen the application of leeches and the supervention of crisis

in such close and constant connexion, that I look on them in the light of cause and effect. Can we explain this? If you look to those diseases which have a tendency to terminate by crisis, you will find that they consist of cases in which there is no great preponderance or excess of irritation in any particular organ. Of this simple typhus is one of the most remarkable examples. When there exists a decided point of irritation in any particular part, the tendency to terminate by crisis is much less. Thus we seldom observe a distinct crisis in cases of acute enteritis or hepatitis, or inflammation of the peritoneum. Whenever we bring on a crisis in any disease in which there is distinct irritation of some particular organ or organs, we generally accomplish our purpose by reducing the local inflammation, and placing the organs in such a state as to give nature fair play. This is a point I have not seen sufficiently dwelt on in any medical work, but it is one of great importance, and which I wish to impress upon your mind. I have seen the application of leeches to the abdomen so frequently followed by a crisis, that I consider it fair to connect these occurrences in the relation of cause and effect. In these cases of the secondary inflammation of fever, it would seem that the tendency of the general disease to terminate by crisis, is prevented by the intensity of a local inflammation, which by its sympathetic irritation keeps up a febrile action. Now if you modify or remove altogether this local affection, you, as it were, reduce the fever to the state of simplicity, and allow the tendency to a critical termination to operate.

A few more observations on this case are necessary. This patient exhibited one peculiar symptom, not generally described in these cases, a very evident pulsation of the abdominal aorta and the vessels which it sends to the viscera of that cavity. It appears that this is a circumstance of common occurrence, and that in most cases where there is acute local irritation the arteries going to the affected part take on an increased action independent of the heart's impulse. Thus, in cases of whitlow there is a manifest excitement observed in the arteries of the corresponding arm. The same thing I believe takes place in enteritis, and we may look on the increased pulsation of the aorta as arising from enteritic inflammation; when you lay your hand on the abdomen of a patient laboring under this disease, you often feel the vessels beating very strongly, *though neither the heart nor the pulse at the wrist is proportionally affected.* I do not say that we are to look on every case of pulsation of the abdominal arteries as the consequence of enteritis or fever, but where we find it occurring thus in fever, we are to conclude that it is indicative of disease in the bowels. We have constantly noticed this pulsation of the arteries of the abdomen to subside after the application of leeches; we have also seen it decline and increase in proportion to the existing disease; and I think we have many circumstances to prove and warrant us in concluding that it accompanies the disease of inflammation of the mucous membrane.

Another point which I look upon as somewhat new, is presented by this man's case. He had incessant thirst, but his desire was for warm drinks, and he refused cold. We may found a part of our diagnosis on this circumstance. In cases of acute gastric inflammation, patients

are harassed by a burning thirst, there is an urgent desire and a constant demand for fluids, but these must be cold, the sufferer generally refuses all others. You will see in any work on toxicology, that in cases of poisoning by corrosive substances, which is only another form of acute gastritis, there is an insatiable desire for cold drinks. In the present instance we find our patient complaining of great thirst, but he prefers warm drinks, and never uses fluids in a cold state, a peculiarity from which I am led to infer that he has no gastritis, but that the inflammation is seated lower down in the digestive tube. When the inflammation is seated, say in the ileum, we have it in a part of the tube less sensible than the stomach. There is the desire for fluids, but not the demand for cold fluids. But when the stomach is the seat of disease, there is both the desire for fluids and relief from the direct refrigeration of the suffering organ. Another important subject for consideration may be noticed in this case. He has hand all through his illness more or less tympanitis, a circumstance to which I am anxious to call your attention, as connected with it is one of the worst errors in practice. From a dangerous habit of prescribing without taking the trouble of searching for causes, and from the universal leaning to specificism in medicine, many practitioners are in the habit of giving the spirits of turpentine when called to attend cases of this kind. Several cases have, indeed, been relieved by this plan of treatment, but I deny that tympanitis occurring in the acute stage of fever has ever been relieved by spirits of turpentine. We are to consider the tympanitis of acute gastro-enteritic fever as one of the consequences of inflammation, and its removal is to be effected only by removing the exciting cause. Can this be done by direct stimulation of an inflamed mucous surface with spirits of turpentine? Certainly not. If we give spirits of turpentine, the patient is purged, (frequently with great violence,) the tympanitis, too, disappears, but the next day we find a manifest increase of fever and thirst, the abdomen is more tender than before, and the tympanitis returns. You may give another dose, but if you do the fever assumes an alarming aspect, marked by the supervention of coma and delirium. Tympanitis we should always consider as one of the symptoms of acute inflammation, and never give turpentine in the commencement of the disease. In the advanced stage of the disease, where turpentine may be employed with benefit we find the tongue soft, and the abdomenal tenderness inconsiderable, and here the safest mode of emplying it is by injection.—*Lond. Med and Surg. Journal, July 20th* 1833, *from Am. Journal.*

14. *On the Efficacy of Dry-Cupping in Various Diseases.*—By R. J. Graves, M. D. Extract from a clinical lecture at the Meath Hospital.—I begin this day's lecture with some observations on dry-cupping, of which you have witnessed the trial in two or three cases at present in hospital. Most of you, I presume, are aware that dry-cupping has been lately recommended to the notice of the profession in a very ingenious and valuable paper published by Mr. Robertson of London; and, as it is a subject deserving of serious and interesting investiga-

tion, involving many considerations of practical importance, it will be necessary to notice it briefly, and offer some hints respecting its applicability to various forms of disease.

Dry-cupping is a remedy not by any means of modern invention; it was known to Hippocrates and Aretæus; and, in succeeding time, among the nations of the European contient and in the British dominions it was verygenerally employed, and formerly enjoyed the reputation of being a very fashionable remedy. Of late, it has fallen very much into disrepute; it is now very seldom employed, though some persons still use it, in hospitals and public institutions, where clinical experiments are conducted on an extensive scale. Mr. Robertson has attempted to revive this practice, and has proved that dry-cupping is a very valuable remedy, possessd of curative powers shared by no other therapeutic agent, and capable of being applied with advantage where the ordinary means are perilous or inadmissible.

Some time ago, Mr. King, of Stephen's Green, related to me the particulars of a case which exhibited, in a very remarkable manner, the benefit derived from dry-cupping. It was a case of hysterical vomiting, in a lady, for which every known remedy had been tried without any favorable result, and which was completely arrested by the application of dry-cupping to the stomach and margins of the ribs. This may appear strange to you, and you may be inclined to ask, how it is that a change in the condition of the integuments of the abdomen can affect the stomach? In reply to this I would ask, in inflammation of the stomach, whether acute or chronic, why is it that the application of leeches to the integuments relieves the gastric affection? In the latter, the result is equally strange as in the former instance; the circulation of the stomach is totally distinct from that of the integuments, and yet we have no remedy so efficient in relieving gastric inflammation as leeches, applied to the integuments of the epigastrium. Taking away blood from the surface produces a change in the circulation of the internal organs; detaining blood in the integuments in the neighborhood of any viscus, acts also on the internal circulation, and effects a corresponding change. Let us investigate this more minutely.

A cupping-glass is applied to some part of the body, and the air contained within it is exhausted by means of a syringe or by heat. In either case the integuments of the part are forced up into the glass by atmospheric pressure, so as to form a hillock, in which a considerable quantity of blood is detained, remaining in the capillaries of the part, and being, as it were, cut off from the general mass of the circulation. The experiments of Dr. Barry have proved the detention of blood in that portion of the integuments submitted to the action of the cupping-glass, and that the quantity so detained does not pass into the general circulation or partake in its changes. Now, if a given portion of skin has, in consequence of morbid action, an unusual quantity of blood thrown into it, and cupping-glasses are applied to the integuments in its vicinity, you draw off a great quantity of blood into the portion which you cup, and that part which presented an unusual quantity, in consequence of morbid engorgement, may be,

pro tempore, drained, and may, during the period of this application, make rapid progress towards health. The same observation holds good when you cup over an internal organ in a state of inflammation. You must be aware of the practice of tying arteries which go to tumors of various kinds, and that the application of the ligature has frequently proved successful in arresting the peculiar inflammatory process by which such morbid developments are accompanied. Now, cupping acts as a kind of temporary ligature on the vessels of the part to which the glass is applied, including even the capillaries; and it is in this way that it tends to prevent the absorption of poisions locally applied.

Having said so much about the application of cupping-glasses, their *modus operandi*, and their action as local applications, let us see how far the principle may be pushed, and also whether this mode may not be applicable to local affections alone, but also act on the general circulation in such a manner as to produce those effects which are commonly attained by different means. Dr. Arnott, in vol. i. p. 574, of his work on the "Elements of Physics," makes the following important observations on this subject:—"Reflection upon these circumstances led me to think that, in certain cases, the beneficial effects of blood-letting might be attainable by the simple means of extensive dry-cupping; that is to say, by diminishing the atmospherical pressure on a considerable part of the body, on the principle of the cupping-glass used very gently, and thus suddenly removing for a time, from about the heart, a quantity of blood, sufficient, by its absence, to produce faintness. The results of trial have been such as to give great interest to the inquiry, and the author's leisure will be devoted to the prosecution of it. An air-tight case of copper, or tin plate, being put upon a limb, and made air-tight by a leathern or suitable collar, tied at the same time round its mouth and the limb—on part of the air being then extracted by a suitable syringe, in an instant the vessels all over the limb become gently distended with blood; and, as the blood is suddenly taken from the centre of the body, faintness is produced, just as by bleeding from a vein. The excess of blood may be detained in the limb as long as desired, for the circulation is not impeded. To produce a powerful effect with a slight deminution of pressure, more than one limb must be operated on at the same time." From this it appears, that if you take the whole arm or leg or thigh of a man and place it under this machine, then exhaust it of air, and detain one or two pounds of blood in the integuments, the same quantity is abstracted from the heart and general circulation, and the effect produced is the same as if you had suddenly drawn blood from the system to this amount. The strongest man will faint if you cup both legs. I think this view of the subject opens new ground in the field of practical medicine. You are all well aware of the effects, the truly beneficial and admirable effects of blood-letting, and you know also, that these depend not so much on the quantity of blood lost as on the impression produced on the general system. If we have to deal with an extensive and violent inflammation, we do not abstract blood by a minute opening, we make a large orifice, or we open a vein in

both arms at the same time, we place the patient in an erect posture and endeavor to produce deliquium. It sometimes happens that the patient faints from fear, or before any considerable quantity of blood has been lost, and this faintness, as Dr. Arnott remarks, answers as well as that which results from venesection. This I can also testify, for I have seen all the good effects of bleeding, produced by the terror with which the operation frequently inspires persons of delicate or nervous temperaments. Now, by the machinery before described, a machinery by no means complicated, you are able to produce with certainty, such a powerful effect on the general vascular system, as to obtain all the benefit derivable from general blood-letting. Dr. Arnott mentions another but more objectionable way of attaining the same purpose, and one which is inferior in efficacy to the mode detailed. If you apply a bandage pretty tightly over the upper part of a limb, suppose for instance round the thighs, so as to prevent the return of blood through the veins, and then put the legs into warm water, the quantity of blood detained in the lower extremities will be such as to make the patient faint. This mode may be useful on some occasions but it is inferior to dry-cupping, and can only be applied to the extremities. There is another and very important point relative to the employment of dry-cupping, which stamps additional value on it from its applicability to cases calculated to excite much solicitude and anxiety in the mind of every practitioner. You have often seen cases of inflammation, in which our sole hope of safety, or even life, depends on checking the inflammatory process, when we stand doubting or perplexed, balancing the possibly fatal effect of blood-letting on a sinking frame, with the slower but, perhaps, more certainly calculated close of an inflammation, which attacks some vital organ, and affects the very sources of existence. If, in such circumstances, we could produce results similar to those which accompany venesection, would it not be a very important desideratum? Now, the employment of dry-cupping holds out to us a fair prospect of attaining this end, of cutting short a menacing inflammation in that particular state of constitution where blood-letting is a perilous experiment, and regulating the errors of morbid action without having recourse to the customary shock of sanguineous depletion. I do not know any better or more valuable auxiliary in the practice of medicine than this, or one which is capable of greater extension and improvement. There is not a single practitioner who does not remember how often he has been forced to bleed when he knew that he was doing so at the risk of his patient's constitution and life; there is no one who has not, on such occasions, anxiously sought some other means of accomplishing the same purpose; and as this is promised by the employment of dry-cupping, I think this matter should become the subject of extensive clinical experiment, and that no time should be lost in proceding to investigate the true properties of a remedy, which is likely to open a new era in medical practice. Cupping-glasses might be made of convenient shapes, for applying them along the inside or outside of the thigh or arm, and might be so large that, with the aid of a syringe, the intended effect could be produced in a few minutes. With regard to

their operation in cases of local disease, I think we cannot extend their use too far. There are many cases of hysterical nuralgia, sometimes affecting the side, sometimes the spine, and other parts, which hitherto we have treated by bleeding, leeches, stupes, liniments, and blisters. Fomentations and liniments some times succeed in removing this affection, so do leeches, but frequently both fail, and we are obliged to blister, which often produces great irritation, without being attended by any decided benefit. Here it is very probable, that we would derive great advantage from dry-cupping in the neighborhood of the affected part. There is one form of this disease to which it is peculiarly applicable. The most annoying thing, perhaps, about which a medical man is consulted, are the head-aches of young ladies. These are varied and numerous beyond conception, generally connected with some menstrual irregularity and derangement of the intestinal canal, and forming a class of disorders which would require a good monograph more than any other I know of. Many practitioners get into disgrace with ladies on this account, and, as a natural consequence, with the community in general. Bleeding here is of very little use, and gives only a temporary relief, or even in many cases aggravates the existing symptoms. The best plan of treatment is to regulate the menstrual secretion, and attend to the state of the bowels. But I will say no more on this subject, for I might lecture on it without end. As to the head-ache, if you leech they get worse afterwards, if you apply cold lotions the same result; the best thing you can do, in my opinion, is to apply dry-cupping-glasses to the back of the neck and between the shoulders.* Let us see what dry-cupping has done in those cases which have been treated with it in hospital. A man of the name of Ryan, who has been a long time in hospital, suffering from violent pains, produced partly by rheumatism and partly by neuralgia, complained of very severe attacks of pain in the lumbar region, lower part of the belly, and thighs, but particularly in the lumbar region, on one side of which the pain and tenderness was excessive. This man had been mercurialized and blistered, he had 100 leeches to the affected parts in eight different applications, he had been stuped repeatedly, he had all manner of liniments and internal remedies I could devise. He was certainly somewhat improved by this treatment, but not so much as I wished. Well, this man has received the most marked benefit and relief of his sufferings from dry-cupping over the seat of the disease.

Another man, named Eustace, who had sciatica, which was cured by acu-puncture and afterwards returned, experienced considerable advantage from this remedy. In the case of a woman above in the fever ward, laboring under bronchitis, we have observed an amelioration of the pectoral symptoms after the application of dry-cupping. It appears to me that cases of pain and tenderness are not the only ones to which dry-cupping is applicable, but that we may employ it

* Dr. Graves has expressed his opinions on this subject more fully in a paper which will appear in the forthcoming number of the Dublin Medical Journal.

also with hopes of success in congestion of internal organs. Cupping over the chest, I think would diminish if not cut short the paroxysms of spasmodic asthma, of tussis senilis, and of the acute suffocative catarrh. In bronchitis with emphysema, it would relieve the congestions of the lungs, and lessen the dyspnœa; and in the violent suffocating bronchitis of children soon after birth it, seems to be particularly valuable from its rapid effects. In the tremendous and fatal dyspnœa which accompanies this affection in children, bleeding and leeching are objectionable, from the danger attendant on them, and from their tedious operation, and are decidedly inferior to the prompt and efficacious agency of dry-cupping, which is free from any danger. You will be convinced that I do not overrate the value and advantages of dry-cupping, when you recollect the case of a man in the hospital who has empyema of the left side of the chest. In this case, which will be spoken of by my colleague, Dr. Stokes, the whole of the cavity of the left pleura is filled with matter; the heart has been pushed to the right side, and the man breaths only through his right lung. Now this man got bronchitis in his only sound lung, and you can easily perceive what danger he was in. It is obvious, that in such cases, from the long duration af the disease, the immense quantity of pus in the pleural sac, and the weakness of the patient's constitution, bleeding could not be employed without much hazard. We had recourse to small doses of tartar emetic and extensive dry-cupping over the chest. The result of this case, which I could not have treated so advantageously a fortnight ago, is very encouraging, for you have seen the relief this poor man obtained. It may seem to you that I am disposed to think too highly of a remedy, the properties of which are at present but little known; but, as I have stated to you before, its properties seem to be analogus to those of general and local bleeding, and it is of the utmost importance to investigate its effects thoroughly, and see if it is capable of the same application, and likely to be attended by similar results, or, if there be any differences in applicability, to know where the one and where the other may be employed with the greatest propriety and success.—*Lond. Med. and Surg. Journ. April 27th,* 1833, *from Am. Journal.*

15. *On the Obliteration of Veins as a Mode of Curing Varices.*—M. Davats, in a learned paper on the different modes of alleviating and curing varicose veins, objects, on various grounds, to all the plans hitherto proposed, and recommends obliteration of the affected vein by a novel procedure. This consists in irritating very slightly two opposite points of the internal surface of a vein, and, at the same time, in keeping those points in contact. A simple sewing needle of any description will answer for the operation, which is thus performed. First the vein is made to swell by means of ligature, is seized by the finger and thumb, and isolated by passing a needle transversely under it; then the anterior, and next the posterior, wall of the vein is pierced perpendicularly with the point of another needle, which is conducted higher up, to pierce a second time first the posterior and next the anterior wall. The last needle is retained in its place by means of a

thread twisted like the figure 8. In five days the thread may be cut, and the needle will fall out of itself. The first needle may be left till it drops spontaneously. In a few hours after the expulsion of the needles, the little wounds will heal kindly.

M. Davats has made several experiments, which prove the safety of the operation, and the rapidity with which a radical cure may be thus performed, complete obliteration taking place by the fifteenth day. In one experiment the operation was performed on two parts of the same vein, leaving a portion between them filled with blood. The blood coagulated, and was absorbed; the vein became obliterated, and no accident occurred.

M. Davats maintans, that compression alone is insufficient to cause obliteration of a vein, and supports his opinion by several experiments, in which the most perfect compression failed to secure this result.

By the operation proposed, the vein is transformed into a cord, white, round, filiform, and analogous to ligamentous tissue. The obliteration extends above and below the point of lesion, as far as the first anastomosing veins, which become sufficiently dilated to give free course to the blood. Beyond the anastomoses the venous tissue assumes its normal state. No trace of inflammation is observed; no clot of blood found in any part; and the cellular tissue surrounding the portion of obliterated vein is found in the most healthy state.—*Edin. Med. and Surg. Journ. Oct.* 1833, *from American Journal.*

16. *On the Presence of Copper in Wheat and several other substances.*—M. Boutigny, by a very interesting and delicate process, has succeeded in discovering the presence of minute portions of copper in cider which had passed through a copper cock,—in cider, in the preparation of which no copper had been employed,—in the factitious waters of Seltzer and Vichy, prepared by several establishments of Paris,—in the water which had served to boil sorrel, artichokes, spinage, and succory, in a cauldron of yellow copper,—in common beef soup, prepared in a copper pot ill-tinned, and in a corresponding mass prepared in the same pot tinned anew,—in three samples of vinegar, and in six of brandy,—in claret, and in *vin de Chably*—and in treacle.

The process which he employed, consists in suspending, by means of a hair, the half of a fine needle in the midst of a liquid previously acidulated with sulphuric acid. The apparatus thus disposed, is placed under a bell-glass, and left by itself in an isolated apartment. As soon as the bar of iron is plunged in the liquid, M. Boutigny conceives that the action commences, though it is not till after six, eight, twelve, or even twenty-four hours, according to the state of the atmosphere, that it becomes apparent. Then the needle looses its metallic lustre. It is commonly at the head that the action commences, proceeding successively through the remainder, and terminating at the base. If the operation be carefully watched, at a certain period the superior half of the needle will be found tarnished, while the other half still retains its metallic lustre. Then will be perceived covering

the needle some air bells, which become larger, and when they have acquired a certain volume, detach themselves, and burst at the surface of the liquid. In from one to three days this disengagement ceases at the surface of the bar of steel, to commence at the inferior part, a curious phenomena, which occurs only in liquids containing a notable quantity of copper, and which may be attributed to two causes, acting, perhaps, simultaneously, electricity, and the coppering of the needle. The latter opposes the disengagement of the gas by an action altogether mechanical, and the needle, in becoming polarized, does not permit the gas to be disengaged except by its negative pole. After this the head of the needle will be the positive pole, and the inferior extremity the negative. Admitting electricity to be the primary cause of the decomposition of the salt of copper, it is very easy to explain the subsequent phenomena. The steel, according to M. Becquerel, is one of the elements of the pile, the acid of the salt of copper is carried to the positive, and the oxide to the negative pole. This, again, is decomposed—the oxygen goes to the iron, which is oxidized, and dissolved in the sulphuric acid, and the copper is precipitated on the steel, covers it entirely, and opposes as mentioned above, the disengagement of the gas. This, however, does not constitute all the action, for the sulphuric acid which M. Boutigny has elsewhere shown to be added merely to render the water a better conductor of the electric fluid, acts on the steel by causing the decomposition of the water, the oxygen of which goes to the iron, while the hydrogen is evolved. The oxide of iron thus formed is dissolved in the sulphuric acid, and forms sulphate of iron, which remains in solution.

To insure the success of the experiment, the following conditions are necessary: 1*st*, a small bar of steel; 2*d*, an acid capable of dissolving the deutoxide of copper, and one of the elements of the steel, after being oxidized at the expense of the water, or of the oxide of copper, and having no action on metallic copper—sulphuric acid, fulfilling all these conditions, ought consequently to be preferred. The other conditions are a liquid, slightly, if at all, viscid, and of the same or nearly the same density as distilled water.

After the appearance of the phenomena enumerated above, there occurs a moment when they cease, which is known by the absence of bubbles, and most frequently by the horizontal position of the cylinder, which has replaced the needle on the surface of the liquid. This cylinder, being carefully lifted, and mixed with a grain of borax and a sufficient quantity of oil to form a paste, is to be placed in a testing-tube, and submitted to the action of a blowpipe, till it acquires a very deep red-brown color. In this state, the tube, when examined by the aid of a good magnifying glass will exhibit several points of metallic copper scattered over its surface.

The results of the facts and experiments narrated by M. Boutigny are, 1*st*, that articles of food and drink prepared in copper vessels, contain almost always more or less of that metal—a fact which renders it desirable that for this metal some other should be substituted; 2*d*, that wine, cider, and wheat, conceal sometimes atoms of this metal, but

only when it is contained in the soil in which the vines, apple trees, and wheat grow; from which we may infer, that presence of copper in vegetables is *not the result of the act of vegetation*, but only of absorption; 3*d*, that the discovery of copper in articles of food and drink raises a question in legal medicine, which renders new investigations necessary, and which, in the meantime, ought to render experiments very circumspect in cases of poisoning by copper.—*Ed. Med. and Surg. Journ. from Journ. de Chimie. Med. March*, 1833, *from Am. Journ.*

17. *On the Transmission of Medicaments into the System by Means of Electro-Galvanism.*—It was stated some time since in one of the English journals, that by the influence of electricity the vaccine virus had been conducted along a wire, and the disease communicated from one person to another as effectually as when vaccination is performed in the ordinary manner. This statmement strongly excited our interest at the time, and we should have then called the attention of our readers to it had it not been coupled with the startling assertain that *intermittent fever*, during the hot stage, had been communicated by the same means to a perfectly healthy person. This last assertion evidently proved too much, and so completely destroyed all the confidence we were disposed to repose in the first, that it appeared to us expedient to wait for some confirmation of the statement, rather than give currency to it by repeating what might prove to be only one of the thousand baseless assertions constantly put forth by careless observers.

It appears from an editorial article in a late number of the *Gazatte Medicale de Paris*, for July 20th, 1833, that Dr. Fabré-Palaprat, previously to the publication of the statements just alluded to, had communicated to one of the Parasian societies a mass of observations relave to the property which he has found the electric fluid to possess, of conducting through our organs a number of active medicaments. It is known that electricity given off from a galvanic pile, has the power of decomposing most bodies, and of transferring their elements to one or other of its poles. This principle or power is the basis of the observations about to be detailed. Dr. Fabré-Palaprat, to whom science is indebted for numerous interesting experiments upon this subject, when he wishes to transfer a medicinal substance to any organ makes use of the voltaic pile, to each pole of which conductors are attached, which serve to transmit the electric current, and at the same time to decompose the substance upon which this current acts. One of the poles is made to communicate with the substance we wish to transfer, the other with a needle, (formed like that used in acupunctuation, of steel, silver, or platina, and of the same shape,) which is inserted into the part of organ to which we wish the medicine applied; for example, the thyroid gland or stomach. In a short time the medicine, let us suppose it to be *iodine* or *quinine*, passes with great rapidity, by some unknown route, to that part of the thyroid gland or stomach which is in communication with the point of the needle. This may be repeated, or the operation prolonged until a sufficient quantity of the iodine or quinine has been applied to the part. In order to prove

that this transmission of the substance takes place, cases of goitre, and intermittent fever are cited, in which a cure of the disease took place from the operation. But it might be said, that in these cases a cure was due, not to the application of the medicine alone, but to other circumstances; as for instance, to the electro galvanism employed, as this is known to exercise a powerful influence over the animal economy! In order to set at rest this point, Dr. F. P. in conjunction with the editor of the *Gazatte Medicale*, undertook a series of experiments. The following is the manner in which they proceeded. Having prepared a vase of porcelain filled with a soultion of the hydriodate of potash, a platina thread was so arranged that whilst one end was plunged in the liquid, the other communicated with the copper pole of a pile of fifty large plates. Another wire of the same metal was made to communicate by one of its ends with the zinc pole of the pile, and by the other with a solution of starch contained in a vase similar to the first. Finally, in order to complete the circle, a finger of the right hand of the operator, was plunged into the iodine solution, and one of the left placed in contact with the platina plate. (We should observe that here, that portion of the conducting circle which passed from the body of the operator to the pile was perfectly dry.) In a few seconds after the circle was formed, in the manner described, several points of a violet tint were observed to form upon the thread which communicated with the starch. These points gradually enlarged, and after a time united one with another, until a complete line of violet hue was formed from the end of the thread to the hand. The appearance of this peculiar color, (violet,) left no doubt whatever as regards the translation of the iodine from the extremity of the conductor attached to the copper pole, to that of the zinc. The following is the chemical explanation of this phenomenon; at the moment that two electric currents disengaged from the two poles of the pile meet, the hydriodate of potash placed in contact with the copper pole is decomposed; the hydrogen is given off; the potash becomes dissolved in the water of the solution, and the iodine powerfully attracted by the positive current, passes rapidly to the zinc pole, as is shown by the violet color of the solution of starch, (the best test for iodine,) placed in communication with this pole. The iodine was selected in these experiments, on account of its presence in extremely minute portions even, being so readily discovered by a solution of starch. Reasoning from analogy, however, there can be no doubt, but that a similar translation of any other body, subject to the action of electricity, would also take place, provided we took the precaution of placing the organ to receive it in communication with that pole of the pile, towards which the electro-chemical disposition of the substance would cause it to proceed. We here repeated these experiments with the iodine, and failed in producing similar effects, notwithstanding every thing seemed to be arranged as it should be. Dr. F. P. has also frequently met with the same results. This failure, however, is but temporary, for with a little patience, and by frequently repeating the operation, we are generally rewarded with success. Dr. F. P. attributes this irregularity of action to the impermeability of the

skin of the operator during the operation. If, as he supposes this irregularity is dependent upon the impermeable nature of the cutaneous tissue of the operator, it is evident that by the employment of metallic needles, this irregularity could not take place, for the conducting circle is here formed of inert matter; the conducting power of which is not affected by any accidental circumstance. However this may be, there rests not a doubt of the posibility of causing the translation of certain remedies to our organs through the medium of an electro-galvanic current, although we are as yet entirely ignorant of the route of this transmission, as well as of the mechanism of the causes which put it in play. Before closing this notice it may be as well to mention certain precautions which we should take in order to insure the success of the operation. In order that decomposition may take place, it is first necessary that the pile be possessed of a certain degree of energy, and further, that the electric current pass uninterruptedly through the different pieces of which the pile consists. These two indications are fulfilled, the first by multiplying with the number and the surface of the metallic parts of which the pile is formed, (of course its entire force will be in proportion to the number of the plates and the extent of their surfaces;) the second by interposing between the plates some body which is a good conductor of electricity. Water for instance, or what is still better, water containing a certain quantity of some salt or acid in solution. Dr. Fabré-Palaprat has also observed, that the decomposed bodies were conducted along a moist conductor only, so that if a part of the conductor of the pile be dry, and the remainder sufficiently moist, the substance experimented upon does not traverse the former, but only the latter. It may be remarked en passant, that as all those portions of the conducting circle, formed either by the needles or by the skin alone, are supposed to be perfectly dry; that there would be but little possibility of the substance operated with, passing in the direction of this portion; for as we have mentioned, this transmission takes pace only when the conductor is moist. But it is positive that the electric current circulates through the different organs into which the needles are inserted, as far even as the extremities of the needles. We may account for the occurrence of this phenomena in the following manner. The whole tract of the needles is imbued with a sufficient degree of moisture, derived from the fluids which circulate in all the tissues, and which are saline or alkeline in their nature, to cause an attraction of the electric current towards them. This moisture, moreover, from its alkaline properties is one of the most powerful conductors we could possibly make use of. Whenever we wish to operate with a substance of a compound nature, an acid for example, we should make use of a pile of feeble energy, that is to say one composed of but few plates, and these of a small size. If we use a powerful one the substance will be decomposed, and we transmit not the substance itself, but its elements. We may operate in this way when we wish to destroy by degress an exuberant tissue, by means of the nitrate of potash. A simple current would be sufficient to disengage the acid from its base, and cause it to pass over to the part operated upon. Another inconvenience likewise attends the applica-

tion of too powerful a current. The quantity of electricity that passes over in this case, may be sufficient to communicate a fatal shock to the part; and if needles are used, their extremities may be heated to incandescence, and thus cauterize more or less deeply the parts into which they are inserted. Dr. Fabré-Palaprat, aware of this, has frequently applied a strong electric current to the needles inserted into a part in which he wished to produce the effects of a moxa.—*American Journal.*

18. *Weight of Man at different Ages.*—From observations made by M. Quetelet, on sixty-three male, and fifty-six female new-born infants, in the Maternité de Saint Pierre, at Brussels, it appears that the mean weight of the former was 3.20 kilogrammes, (6.536lbs,) while the length, by Chaussier's mecometer, was 0.496 metres, (1 foot, 6 inches, 3 lines;) and of the females, the mean weight was 2.91 kilog. (5.923lbs,) the length 0.483 metres, (1 foot, 5 inches, 10 lines.) Whence it is inferred that at birth there is an inequality in the weight and size of the two sexes—the males having the advantage in both.

Chaussier seems to have been the first who remarked that the infant, presently after birth, begins to lose some of its weight. M. Quetelet, from seven series of observations, extending in each case to the seventh day, has confirmed M. Chaussier's remark, and shows that the infant does not begin to grow perceptibly till after the first week.

M. Quetelet gives a table of the corresponding weights and statures at the different ages. We extract a few of them, by way of specimen:—

Ages.	Males.		Females.	
	Height.	Weight.	Height.	Weight.
	m.	*k.*	*m.*	*k.*
At Birth	0.500	3.20	0.490	2.91
1	0.698	9.45	0.690	8.79
3	0.864	12.47	0.852	11.79
6	1.047	17.24	1.031	16.00
10	1.275	24.52	1.248	23.52
20	1.674	60.06	1.572	52.28
30	1.684	63.65	1.579	54.33
40	1.684	63.67	1.579	55.23
50	1.674	63.46	1.537	56.16
70	1.623	59.52	1.514	51.52

In order to render these results still more striking, the author has delineated, by two curves, the course which the weight takes in either sex. The ordinates of the curves expressing the weights, and the abscisses the ages. It is thus seen at a glance, that at any given period the man is generally heavier than the woman. About the age of twelve, however, it may be observed that the weights of both sexes are generally equal. This circumstance must, of course, be imputed to the earlier approach of puberty in the female.

Some of the other conclusions of M. Quetelet are curious. Man

attains his maximum weight about the age of 40, and begins to lose weight very sensibly towards 60. Woman is not at her maximum weight till towards 50. Between 18 and 40, the period of her fecundity, she does not acquire any very perceptible increase of weight.

Both man and woman, at the period of their complete development, weigh almost exactly twenty times as much as they did at birth. Their height at the same period is about 3¼ times what it was at birth.

In their old age, both man and woman have lost 6 or 7 kilogrammes of their weight, and 7 centimetres of their height.

During the growth of both sexes, it may be stated that the squares of the weights are as the fifth powers of the heights.

After the development is complete in both sexes, the weights are very nearly as the squares of the heights: whence it may be inferred that the increase in the height is greater than the transverse increase of the body, comprehending both its breadth and depth.

The mean weight of an individual, without reference to sex or age, is 44.7 kilogrammes, (91.336lbs:) or, if sex be taken into account, it is 47 kilogrammes, (96.015lbs:) for men, and 42.5 kilogrammes, (86.-831bls.) for woman.

According to the observations of the late M. Tennon, of the Institute, (which observations are given by way of supplement to M. Quetelet's paper,) the Laplanders and the Patagonians present the two extremes of man's stature. The former commonly measure from 4 feet to 4 feet 6 inches, 4 feet three being their mean height, and their women are scarcely less. The Patagonians measure from about 5 feet 6 inches to 6 feet 3, and their women are generally 7 or 8 inches shorter.

The tallest men in Europe, M. Tenon thought, were in Saxony. But he added, that the climate or locality had less to do with the stature of men than their race or variety. Close by the Saxons, for example, we find the Silesians, who are a short people; and the Patagonians are the Pecherais, a people much inferior in height. In Savoy also, and particularly about *La Haute-Maurienne*, extreme varieties have been noticed. Yet it can scarcely be doubted but that the climate, the nature of the soil, the sort of government, the state of civilization, and the comforts, or the contrary, of each people, have much influence in determining a national stature. This position has been strongly urged by M. Villermé, in his *Memoir sur de l' Homme;* and very interestingly stated by W. F. Edwards, in his *Caracteres Physiologiques des Races.—Lond. Med. Gaz. September*, 1833, *from Am. Journal.*

19. *Lunatics in New-York.*—At a late meeting of the New-York Medical Society, the subject of lunacy was under consideration. It was ascertained that the number of lunatics and idiots in the State amounted to about *three thousand*, that about three-fourths of these are poor, and that the Asylum at Bloomingdale, and the private retreat of Dr. White of Hudson, can accomodate at most but three hundred—thus leaving about two thousand seven hundred of these wretched beings, in that single State, almost entirely destitute of medical or moral treatment, confined in poorhouses or jails, or private abodes, or wandering about the country, and treated, too often, more like brutes than human beings. This is indeed a melancholy picture;

and when we refer to the records of our well-managed asylums, and see how all are treated with kindness, and how many have their reason restored to them, we cannot but believe that the Legislature of New York will respond cheerfully and speedily to the call of the Society for money to erect a suitable edifice for the reception and treatment of these unfortunate beings.

The Legislature of the State of Maine has established, at its recent setting, a noble example in this species of benificence. It has appropriated 20,000 dollars for the establishment of an asylum for the insane, on the condition that other 20,000 dollars shall be raised by private subscription. A single individual in that State promptly subscribed half this sum, and no doubt can exist that the remainder will be speedily obtained.

Probably there is no department of science, no form of humanity, in which greater advances have been made of late years, than in the medical and moral management of the insane. When we contrast the spacious and airy apartments and grounds of our asylums, with the dark, and narrow, and dirty cells, in which, twenty years ago, the best accommodated of these poor creatures were immured—their neat and comfortable dress, with their former rags and nakedness—their wholesome food, with their former rations—and above all, the kindness and affection which is shown to them now, with their utter neglect in the days when they were excluded from the privileges and the society of men, we find ourselves shuddering at the thought of what we *have seen*, and lost in admiration of what we *now see*.

Wherever the christian religion exists, we find the same rapid advances making towards the accomplishment of the great purposes of humanity. It seems as if the miracles of our Saviour were meant as prototypes of what his religion was to accomplish. It is by the influence of this religion on the march of science and philosophical discovery, that, by all christian nations, the winds and the waves have been rebuked—that man is enabled to ride out the storm upon the ocean, as if it were hushed, and, like Peter of old, to walk upon the sea as on dry land. Institutions, too, throughout christendom, are eitheir already established or fast rising up, where the blessings of sight are bestowed upon the blind, and of hearing upon the deaf;—where the dumb are made to speak—lepers are cleansed, the sick are healed, the lame are made to walk, the maimed restored to their integrity, and where evil spirits even are cast out, and the poor lunatic receives back again his lost reason.

Among the improvements in the mode of managing the insane, that have been recently adopted with marked success, we have been particularly impressed with the wisdom of the plan which furnishes them *employment*. Among the numerous inmates of our asylums, we apprehend there are few who have always been accustomed to regular bodily labor, and the degree of mental action that is usually required by occupations of this description. Employment is the door that keeps health in the body and reason in its proper abode. Take it away, and the one will certainly take its flight, and the other be not unlikely

to follow. It seems rational, therefore, that when the means resorted to in the treatment of the insane, have, to a certain degree, been successful, the furnishing such occupation as has a plain and palpable object is a wise and promising measure. A familiar account of its adoption in Europe will be found in the following extract from one of the letters of Mr. Willis.

"Two of the best conducted lunatic asylums," says he, "in the world, are in the kingdom of Naples—one at Aversa, near Capua, and the other at Palermo. The latter is managed by a whimsical Sicilian baron, who has devoted his time and fortune to it, and, with the assistance of the Government, has carried it to a great extent and perfection. The poor are received gratuitously; and those who can afford it, enter as boarders, and are furnished with luxuries according to their means.

The hospital stands in an airy situation in the lovely neighborhood of Palermo. We were received by a porter in a respectable livery, who introduced us immediately to the old baron—a kind-looking man, rather advanced beyond middle life, of manners singularly mild and prepossessing. '*Je suis le premier fou,*' said he, throwing his arms out. as he bowed on our entrance. We stood in an open court, surrounded with porticoes and lined with stone seats. On one side of them lay a fat indolent-looking man, in clean grey clothes, talking to himself with great apparent satisfaction. He smiled at the baron as he passed, without checking the motion of his lips, and three others standing in the door-way of a room marked as the kitchen, smiled also as he came up, and fell into his train, apparently as much interested as ourselves in the old man's explanation.

The kitchen was occupied by eight or ten people all at work, and all, the baron assured us, mad. One man of about forty, was broiling a steak with the gravest attention. Another who had been furious till employment was given him, was chopping meat with violent industry in a large wooden bowl. Two or three girls were about obeying the little orders of a middle-aged man, occupied with several messes cooking on a patent stove. I was rather incredulous about his insanity, till he took a small bucket and went to the jet of a fountain, and getting impatient from some cause or other, dashed the water upon the floor. The baron mildly called him by name, and mentioned to him, as a piece of information, that he had wet the floor. He nodded his head, and filling his bucket quietly, poured a little into one of the pans, and resumed his occupation.

We passed from the kitchen into an open court, curiously paved, and ornamented with Chinese grottoes, artificial rocks, trees, cottages and fountains. Within the grottoes reclined figures of wax. Before the altar of one, fitted up as a Chinese chapel, a mandarin was prostrated in prayer. The walls on every side were painted in prospective scenery, and the whole had as little the air of a prison as the open valley itself. In one of the corners was an unfinished grotto, and a handsome young man was entirely absorbed in thatching the ceiling with strips of cane. The baron pointed to him and said he had been incurable till he had found this employment for him. Everything about

us, too, he assured us, was the work of his patients. They had paved the court, built the grottoes and cottages, and painted the walls under his direction. The secret of his whole system, he said, was employment and constant kindness. He had usually about one hundred and fifty patients, and he dismissed upon an average two-thirds of them quite recovered.

We went into the apartment of the women. These, he said, were his worst subjects. In the first room sat eight or ten employed in spinning, while one infuriated creature, not more than thirty, but quite gray, was walking up and down the floor, talking and gesticulating with the greatest violence. A young girl of sixteen, an attendant had entered into her humor, and with her arm put affectionately round her waste, assented to every thing she said, and called her by every name of endearment while endeavoring to silence her. When the Baron entered, the poor creature addressed herself to him, and seemed delighted that he had come. He made several mild attempts to check her, but she seized his hands, and with the veins of her throat swelling with passion, her eyes glaring terribly, and her tongue white and trembling she continued to declaim more and more violently. The baron gave an order to a male attendant at the door, and beckoned us to follow, led her gently through a small court planted with trees, to a room containing a hammock. She checked her torrent of language as she observed the preparations going on, and seemed amused with the idea of swinging. The man took her up in his arms without resistance, and laced the hammock over her, confining every thing but her head, and the female attendant, one of the most playful and prepossessing little creatures I ever saw, stood on a chair, and at every swing threw a little water on her face as if in sport. Once or twice the maniac attempted to resume the subject of her ravings, but the girl laughed in her face and diverted her from it, till at last she smiled, and dropping her head into the hammock, seemed disposed to sink into an easy sleep.

We left her swinging, and went out into the court, where eight or ten women, in the gray gowns of the establishment, were walking up and down, or sitting under the trees, lost in thought. One, with a fine intelligent face, came up to me and curtsied gracefully without speaking. The physician of the establishment joined me at the moment, and asked her what she wished. 'To kiss his hand,' said she, 'but his looks forbade me.' She colored deeply, and folding her arms across her breast, walked away. The baron called us, and in going out I passed her again, and taking her hand, kissed it and bade her good-by. 'You had better kiss my lips,' said she, 'you will never see me again.' She laid her forehead against the iron bars of the gate, and with a face working with emotion, watched us till we turned out of sight. I asked the physician for her history. 'It was a common case,' he said. 'She was the daughter of a Sicilian noble, who, too poor to marry her to one of her own rank, had sent her to a convent, where confinement had driven her mad. She is now a charity patient in the asylum.'

The courts in which these poor creatures were confined, opened upon a large and lovely garden. We walked through it with the baron, and

then returned to the apartments of the females. In passing a cell, a large majestic woman, striding out with a theatrical air, commenced an addres to the Deity, in a language strangely mingled of Italian and Greek. Her eyes were naturally large and soft, but excitement had given them additional dilation and fire, and she looked a prophetess. Her action, with all its energy, was lady-like. Her feet, half covered with slippers, were well formed and slight, and she had every mark of superiority both of birth and endowment. The baron took her by the hand with the deferential courtesy of the old school, and led her to one of the stone seats. She yielded to him politely, but resumed her harangue, upbraiding the Deity, as well as I could understand her, for her misfortunes. They succeeded in soothing her by the assistance of the same playful attendant who had accompanied the other to the hammock, and she sat still, with her lips white and her tongue trembling like an aspen. While the good old man was endeavoring to draw her into a quiet conversation, the physician told me some curious circumstances respecting her. She was a Greek, and had been brought to Palermo when a girl. Her mind had been destroyed by an illness, and after seven years' madness, during which she had refused to rise from her bed, and had quite lost the use of her limbs, she was brought to this establishment by her friends. Experiments were tried in vain to induce her to move from her painful position. At last the baron determined upon addressing what he considered the master passion in all female bosoms. He dressed himself in the gayest manner, and, in one of her gentler moments, entered the room with respectful ceremony, and offered himself to her in marriage! She refused him with scorn, and with seeming emotion he begged forgiveness and left her. The next morning, on his entrance she smiled—the first time for years. He continued his attentions for a day or two, and after a little coquetry, she one morning announced to him that she had reconsidered his proposal, and would be his bride. They raised her from her bed to prepare her for the ceremony, and she was carried in a chair to the garden, where the bridal feast was spread, nearly all the other patients of the asylum being present. The gaiety of the scene absorbed the attention of all; the utmost decorum prevailed; and when the ceremony was performed, the bride was crowned, and carried back in state to her apartment. She recovered gradually the use of her limbs, her health is improved, and excepting an occasional paroxysm, such as we happened to witness, she is quiet and contented. The older inmates of the asylum still call her the bride; and the baron, as her husband, has the greatest influence over her.

While the physician was telling me these circumstances, the baron had succeeded in calming her, and she sat with her arms folded, dignified and silent. He was still holding her hand, when the woman whom we had left swinging in the hammock, came stealing up behind the trees on tiptoe, and putting her hand suddenly over the baron's eyes, kissed him on both sides of his face, laughing heartily, and calling him by every name of affection. The contrast between this mood and the infuriated one in which we had found her, was the best comment on the good man's system. He gently disengaged himself, and apologi-

zed to his lady for allowing the liberty, and we followed him to another apartment. It opened upon a pretty court, in which a fountain was playing, and against the different columns of the portico sat some half dozen patients. A young man of eighteen, with a very pale scholar-like face, was reading Ariosto. Near him, under the direction of an attendant, a fair, delicate girl, with a sadness in her soft blue eyes that might have been a study for a *mater dorolosa*, was cutting paste upon a board laid across her lap. She seemed scarcely conscious of what she was about, and when I approached and spoke to her, she laid down her knife and rested her head upon her hand, and looked at me steadily, as if she was trying to recollect where she had known me. 'I cannot remember,' said she to herself, and went on with her occupation. I bowed to her as we took our leave, and she returned it gracefully but coldly. The young man looked up from his book and smiled, the old man lying on the stone seat in the outer court rose up and followed us to the door, and we were bowed out by the baron and his genteel madmen as politely and kindly as if we were concluding a visit to a company of friends."—*Boston Medical and Surgical Journal.*

II.—ORIGINAL.

The Western Journal.

E Sylvis Nuncius.

Cincinnati, April 1, 1834.

I. The Medical College of Ohio.

The present number completes the 7th volume of our Journal, and the present month terminates the 7th year since it began. Throughout the whole of this *climacter*, the condition and prospects of the *Medical College of Ohio*, established, as one of its late circulars informs us, in Cincinnati, have been a subject of professional conversation, controversy, and publication. In all that considerable period, moreover, the name of the Editor of this Journal has, in various ways, been associated with the complaints against and from the faculty and trustees of the institution; but his readers will do him the justice to acknowledge, that, with the exception of a few *documents* and mere *notices*, inserted in some of the numbers of the present volume, he has not introduced the subject into its pages. On the contrary, he has generally given publicity to the number of the different classes, the catalogue of graduates, and other items, indicating the progress of the institution.

This forbearance, for which, perhaps, no sufficient reason could at any time be given, has kept the readers of the Journal in ignorance of the policy and progress of the school; when one object of medical journals should be to disseminate correct

information concerning medical institutions. The Editor is determined, hereafter, to advise the distant reader, from time to time, of the character and advantages of our Medical College; and to gratify their curiosity as to recent transactions, he proposes to devote a portion of the present number, to the publication and examination of several documents.

As far back as the year 1827, the Third District Medical Society sent a circular to the other Societies of the State, expressing the opinion, that the professorships in the College were not adequately filled. In 1828, the Medical Convention, consisting of delegates from the District Societies, passed a resolution, desiring the General Assembly to do something for the improvement of the School. In the year 1832, the Third District again acted on the subject, and a majority of all its members, by a unanimous vote, a second time directed the attention of the other Districts to the matter, and likewise sent a memorial to the General Assembly. In the following January, 1833, the State Medical Society held its second session, in Columbus, and adopting the resolutions of the Third District, united with them in an application to the Legislature, for a revision and enlargement of the board of Trustees. It was on these joint petitions, that a resolution was passed, authorizing the Governor to appoint a Board of five Commissioners. As already announced in a former number of the Journal, they met at the College edifice, in the month of April last, and made an inquiry.

On the manner of this inquiry two remarks may be made. First. They did not appoint an attorney, or advocate, on the part of the State, nor would they allow the officers of the Society to employ one; while the corporation, embracing seven lawyers, were ably represented by two of them. Secondly. They would not allow testimony to be given to show the general character and standing of the School, compared with other institutions, (clearly the most important part of the inquiry) and in this they conformed to the expressed wishes of the Trustees. Mean while, the law incorporating the District Society, was repealed, dissolving the bodies which had presented the subject to the Legislature, and leaving,

of course, no prosecutor. Dr. Martin, however, the President of the Third District Society, and some of its members, who had been a standing Committee on that subject, resolved to step forward, and when the Commissioners were prepared for business, he laid before them the following:

Charges and Specifications.

Charges and Specifications preferred against the Trustees of the Medical College of Ohio, at Cincinnati; and the Faculty, or several of them, of the said College, by the Committee of the Third Disirict Medical Society of the State of Ohio.

Charge First.

That for several years past the general policy of the Trustees of the Medical College of Ohio, in the city of Cincinnati, has been marked by partiality and prejudice, and indicates an indifference to the reputation and welfare of said College, and to the advance of medical science therein.

Specifications.

1st. That in the year 1822, when the said Trustees were appointed, and assumed the charge of the said Medical College, they filled up the vacant Professorships with unknown and incompetent men, to the manifest injury of the character of the institution; and in defiance of public benefit and public opinion, upheld them in office until forced to abandon them by the appearance of a rival institution, in 1831.

2d. That whereas, pupils should have been attracted to the said College by the abilities of its teachers, and the advantages of its circumstances and location alone, the said Trustees, for a period of six or seven years, held the price of tuition lower than in any similar school in the United States.

3d. That in consequence of this undercharging by the said Trustees, the character of the School was degraded abroad, insomuch that the Medical Faculty of Transylvania University, in Kentucky, refused to recognise a course of lectures in the said Medical College of Ohio, as equivalent to one in their own institution; whereby the state of Ohio was injured, and her means of advancing the medical science impaired.

4th. That in the summer of 1831, when the weakness of the said Medical College of Ohio, had invited the establishment of a rival School in the city of Cincinnati, under the auspices of the Miami University, and not until then, the said Trustees displaced three of their professors, created an eighth professorship, to wit, that of Clinical Medicine, and adopted five of the Professors from the said rival school. That Dr. Pierson, a brother-in-law of a member of the Board of Trustees, was retained, and was transferred from the chair of Materia Medica to that of the Institutes of Medicine: that he declined accepting the said chair unless the said Trustees would allow him one year for preparation: that this unjustifiable favor was granted to him by

the said Trustees, although he was a citizen of the state of New York, and possessing no peculiar claims upon the said Trustees. That the professorship of Clinical Medicine could not, in the opinion of the class, and of a majority of the Professors, be sustained as a separate chair; but that the chair of the Institutes might have been properly associated with it. That the then professor of Clinical Medicine, who lectured extemporaneously upon the Institutes while the said Dr. Pierson was preparing himself in New York, would willingly have taken the chair formed out of these two, and have continued in the School, but that they adhered to Dr. Pierson, and the former was forced to resign.

5th. That as soon as this was brought about, the said Trustees unwisely abolished both these chairs, and then, in order to bring Dr. Pierson back to the chair of Materia Medica, it was proposed to expel Dr. Henry from the chair of Obstetricks, and give that chair to Dr. Eberle, who refused to accept it: that the said Trustees then adopted a different plan, to wit, they dismissed Dr. Henry from the chair of Obstetrics, transferred Dr. Moorhead to the same chair, from that of Theory and Practice, thus providing a place for Dr. Eberle, whereupon Dr. Pierson was restored to the chair of Materia Medica.

6th. That the person in whose favor all these violent movements were made, was then, and had been from the time of his first appointment, in 1825, a citizen of the state of New York, whence no pupils come to the said Medical College of Ohio; that he was unknown to the physicians of the West, and possessed no peculiar claims upon public attention as a lecturer.

7th. That the gentleman who was compelled to resign, by this adherence of the said Trustees to the interests of Dr. Pierson, had been 43 years in the West; had lectured with the highest approbation to nearly a thousand pupils in the Transylvania University; was on good terms with the Faculy of said Institution, and his talents and services held in great estimation by them; was extensively and advantageously known over the valley of the Mississippi, and indeed throughout the United States; was at this time recommended by the graduates of the Medical College of Ohio for the chair of the Theory and Practice of Medicine, and was the founder of said College, and of the Hospital and Asylum connected with it.

8th. That the gentleman who was improperly expelled, for the purpose of making the aforesaid transfers, that Dr. Pierson who had been eight months preparing himself for the chair of the Institutes, might be brought back to the chair of Materia Medica, was a native of the West; had resided in Kentucky and in Missouri, and was known and esteemed for his talents and general respectability in those states, and also in Illinois and Indiana; that he was at the time of his dismission a permanent citizen of Cincinnati; that at the said time, he had, in the said College, lectured to the decided satisfaction of the class, a majority of whom petitioned against his removal; that he had been one of the professors of the Miami School, which by compromise was united to said College, and that in expelling him without any cause assigned the said Trustees acted unwisely, and unjustly, and violated the faith of said College: That this conduct gave offence to the pupils,

especially those from Kentucky, his native state, and created enemies to the College, not only in that state, but wherever he was known.

9th. That during the session of 1831–2, in which Dr. Pierson was absent, but when Drs. Drake and Henry were connected with the school, although the aggregate price of the tickets was advanced from $64 to $100, the number of students was greater than it had ever been before; and that, in the succeeding session, when Dr. Pierson was restored, and the price of tickets was but $90, the number of students was reduced to about one half of what it had been the preceding year, through the mismanagement and favoritism of the said Trustees.

10th. That the said Board of Trustees have rendered the present organization of the said Medical College defective, by the improper abolition of the chair of the Institutes.

11th. That some of the present Professors are manifestly deficient in qualifications; and that the whole, taken together, do not form such a body of teachers as to entitle the School to the confidence of the profession and of the public at large, and that whereas, from the population, water and other communications of the city of Cincinnati; its central situation in the West, and the opportunities which might be afforded by the Hospital and Asylum for the study of practical medicine and morbid anatomy, the said city is better calculated to support a great Medical School than any other city west of the mountains:—The said Trustees, by their partiality, prejudice, and illiberal management of the professorial department have interrupted the harmony, checked the progress, and weakened the popularity of the School committed to their care; and that it is confidently believed that by proper exertions on the part of the said Trustees, all the professorships might have been, and might now be filled with men of superior abilities and extensive reputation, who would soon render the said Medical College the second or third school in the Union, in point of numbers.

12th. That the present Board is incompetent to the charge and superitendance of a medical school, as it contains but one medical man.

13th. That an influential member of said Board of Trustees is an avowed and practical advocate of what is generally termed the Thompsonian, or steam system of medicine, and should not, therefore, preside over a College for regular, scientific instruction, nor be allowed to subscribe the diplomas of its graduates.

14th. That the abolition of the chair of the Institutes, referred to in specification 10th, was totally uncalled for, and had solely for its object to remove Dr. Pierson from that chair, for which he had confessed himself unprepared, in order that a pretext might be raised for re-instating him in the easier chair of Materia Medica.

15th. That the said Trustees have mismanaged the Library, and greatly limited its usefulness, by omitting and refusing to establish a library ticket, to be purchased by physicians and by students.

Charge Second.

That the Board of Trustees of the Medical College of Ohio have

been guilty of gross negligence in the management of the funds of the state committed to their hands for the benefit of the said College.

Specifications.

1st. That in planning the edifice erected at the expense of the state for the Medical College of Ohio in Cincinnati, in the year 1826, the rooms destined for chemical purposes were so badly and improvidently planned, that in 1832 the said Trustees were compelled to take down and rebuild the whole interior of the same, at an expense to the state of several hundreds of dollars.

2nd. That in commencing the additional edifice in 1831, belonging to the said College, the said Trustess did not, as was their duty, nor did the Faculty of the said College, ever examine the plan of the said additional edifice; in consequence of which neglect, the state was put to the charge of several expensive alterations in the progress of the building.

CHARGE THIRD.

That the Board of Trustees of the Medical College of Ohio, contrary to their known duty, permit circulars and reports on the state of the institution to go forth to the General Assembly and to the world, which they have never seen and examined—to wit, the annual circular of the Institution dated the 27th of May, 1832; the annual report of a committee of the said Board of Trustees to the General Assembly, on the 18th of the ensuing December, and the memorial of the said Board to the General Assembly, on the —— of February, 1833.

CHARGE FOURTH.

That the Trustees of the Medical College of Ohio, have injured the public estimation of the said College abroad and at home, and have greatly impaired its utility and respectability, by uncalled for partiality towards men, who, as professors, had no claims upon them, and by unjust persecution of able, respectable, and unoffending individuals, whom they were bound in good faith to protect.

Specifications.

1st. In fraudulently endeavoring to break up a rival institution, by detaching some of its professors from their pledges to their associates.

2nd. In granting an indulgence to Dr. Pierson, altogether unprecedented, in reference to a chair purely *intellectual.*

3d. In countenancing and encouraging slanderous statements in reference to the lectures of Dr. Henry, the Professor of Obstetrics, without, on any occasion having manifested the least disposition to defend his character, or even to give him prompt notice of the existence of such statements.

4th. In encouraging or permitting the attack which the Township Trustees made on Drs. Drake and Henry in January, 1832.

5th. In conniving at and encouraging the attack make upon the Pro-

fessor of Obstetrics, in the month of February, 1832, by the Township Trustees.

6th. In unjustly retaining for seven days the letter of the Township Trustees, impeaching the character of a Professor of the College, without communicating to that Professor, or to the Faculty, the letter, or the fact of its existence.

7th. In suffering the false statement of the Township Trustees that "they had never instituted an inquiry into the subject," to pass without contradiction.

8th. In having contemplated, early in the session, the abolition of some of the professorships which they had established during the preceding summer, against the wishes of the students and the public, for the purpose of unjustly depriving a professor or professors of their offices.

9th. In having extorted or frudulently obtained the consent of a majority of the Faculty to the proposed reduction of the professorships—whereby their contract, compromise, or consolidation during the preceding summer, with the professors of a rival school, was openly violated, to the bad name and manifest injury of the College over which they presided.

10th. In retaining one Professor, from private family consideration, whose chair they had abolished, and expelling another, without cause assigned, whose chair was preserved, and that in opposition to the expressed wishes of a large majority of the class then in attendance.

Charges against the Faculty of the Medical College of Ohio at Cincinnati.

1st. That the said Faculty refused and omitted to establish a hospital ticket, for students who wished, during the eight months in which the said Medical College was not in session, to attend the practice at the Hospital and Asylum at the city of Cincinnati, while they admitted their private pupils gratuitously, thus rendering these public charities a *bonus* to young men to study with them, and deprived the College of a considerable source of revenue, specifically pledged in the law establishing these Infirmaries, for the purchase of Books, and Philosopical apparatus for the said Medical College.

2nd. That the said Faculty, whose duty it was, have neglected to give proper attendance upon the lunatics of the Asylum, except when they were ill of some malady of a common kind, superinduced upon their insanity, alleging that the building provided by the Township Trustrees and the general arrangements for the care and keeping of the lunatics, were such that no hope of curing their lunacy could reasonably be entertained; but, at the same time, that the said Faculty neglected, as was their duty, to report the same to the General Assembly.

3d. That the said Faculty have neglected and do still neglect to render the Hospital and Asylum a school for the study of morbid Anatomy, to the extent which it is practicable, whereby they have

lost, and still do lose to the said College, the chief advantage which these Infirmaries might be made to confer upon the pupils thereof.

4th. That the said Faculty have sought to discourage private lectures to students of Medicine, both in summer and in winter, by others than themselves, whereby the attractions which Cincinnati might exert upon students have been diminished, just so far as the said Faculty have succeeded in the suppression of such lectures: That this is the policy which the said Faculty have pursued in defiance of the example of the best regulated medical institutions of other places; the more a Medical College can surround itself with private lectures, the greater being its popularity and patronage.

5th. That Dr. Staughton, as Dean of the said Faculty, failed to call a Faculty meeting, as was his duty, when in February, 1832, the Trustees of the College sought an approval of their scheme of reducing the number of professorships to six.

6th. That the Surgical Professor of the said Faculty has been guilty of neglect or mal-practice in the Hospital in the two cases of compound fracture, within six months of each other, in both of which, death apparently took place in consequence of said neglect or mal-practice.

7th. That the study of Practical Anatomy has not been conducted in a manner to offer the greatest inducements to medical students, or equal to the advantages which the said College might have afforded.

8th. That a majority of the said Faculty attended the last semi annual meeting of the First District Medical Society, and by their personal influence, dishonorably exerted, endeavored to obtain from said Society a vote of approbation of the conduct of the Trustees of the said College.

Additional Charges against the Trustees and Faculty.

1st. That in the late annual report to the General Assembly of Ohio, the Trustees and Faculty of the Medical College of Ohio have knowingly put forth false statements as to the number of pupils in attendance upon the lectures, as to the causes of their diminution from what they were the preceding year, and as to the causes of the difficulty of prosecuting *post mortem* examinations in the Hospital and Asylum.

2nd. That during the time when the Professor of Clinical Medcine was *de facto* a member of the College, one of the said Trustees put forth a false, insiduous, and anonymous publication in the newspapers, designed to reconcile the public mind to the resignation which the unjust policy of the said Trustees had rendered unavoidable to the said Professor.

3d. That after having in the most unjustifiable manner compelled the Professor of Clinical Medicine to resign his chair, they proceeded to justify their conduct by traducing his character; thereby tarnishing the good fame of the institution, and converting his friends into enemies of the school, to the injury of its prospects.

4th. That the Trustees and Faculty of the said Medical College have attempted to prejudice the fairness of the present Legislative investigation, and to screen their own doings from too close a scrutiny, by falsely and willingly attributing to the influence, maliciously exerted, of a single interested individual in the city of Cincinnati, the dissatisfaction of the physicians throughout the state, with the management of the said College; alleging that said physicians are the subservient tools of the said individual; thus giving just cause of offence to the profession in Ohio, and converting them into enemies of the said Medical College.

Signed, on behalf of the late
Third District Medical Scociety
of the State of Ohio,
JOSHUA MARTIN, M. D.
Late President T. D. M. Socy.

Cincinnati, April 12, 1833.

The Commissioners continued in session for nearly three weeks, and examined between 40 and 50 witnesses. In the succeeding December, they made their Report to the General Assembly; and accompanied it with several hundred manuscript pages of testimony. How far this testimony went to sustain the Charges, they did not say; but leaving that matter to the General Assembly, recommended an enlarged Board of Trustees and a new Organization. The Report and the testimony were referred to a Select Committee, who, after some weeks, made a Report on the same; which was promptly adopted with one dissenting voice. *It was not discussed, nor was the testimony to which it related ever read before the Senate.* Thus that honorable body gave its vote in total ignorance of the testimony, which, at a heavy expense to the state, it had ordered to be taken. Why it should have done so, is and will, perhaps, remain unexplained, but the fact deserves to be recorded, as a part of the legislative history of the institution.

From beginning to end, the Report is laudatory of the Trustees and the various Faculties—commendatory of the establishment—and abusive of the Editor of this Journal, whose name is introduced more than twenty times! It is certainly quite a distinction to be made thus the subject of an elaborate and lengthened state paper, by a Committee of the most dignified branch of our Legislature! Towards the close of this Report on the conduct and character of "Drake," as the

Committee courteously designate him, they declare "*That the Charges against the Agents of the State are* NOT SUSTAINED *by the evidence;*" consequently the "Agents of the State" are acquitted, and *one* of the fifty witnesses singled out for calumniation, within the very walls, where he once personally made those explanations to the General Assembly, which led to the establishment of both the College and the Hospital!

What influence could have induced the Committee to palm upon the Senate a declaration so *foreign to the fact*, as that in the quotation just made, will perhaps never be found out. The most charitable conclusion would be, that the Report was prepared for them in Cincinnati. If they wrote it themselves, they either omitted an examination of the testimony, or did that which is less excusable. This is a grave accusation, but the Editor proposes to make it good by a reference to the unread and unpublished evidence now buried in the Archives of the Senate. He will proceed then to state what charges *were* sustained, by that testimony.

Before proceeding, however, to this object, the Editor would remark, that it is difficult to understand precisely the extent of the assertion, that the "charges against the agents of the State have not been sustained." The sentence is artfully framed. First. The Faculty are not the agents of the State, and consequently are not embraced in this sentence of acquittal; and still, no doubt it was intended by the Committee that it should be so construed as to include them. Secondly. Do they mean that *all* the charges, or only a part of them, were not sustained? If the former, the assertion is one, perhaps, unequalled in the annals of legislation, for recklessness of fact. They could not *say* that *all* the charges were not sustained, but desirous of making that impression, without committing themselves, they say the "charges are not sustained." The Editor does not pretend that *all* the charges were supported, but he knows that many of them were, and will proceed to show it. He will refer to them by numbers, to avoid repetition.

In reference to the first specification of the first charge, it appeared in evidence, that the Trustees appointed Dr. Cobb,

of the state of Maine, to the chair of the Theory and Practice of Medicine, within a year or two after his graduation, when he was a very young, and an entirely inexperienced physician; and that he himself, from a consciousness of want of qualification for that practical chair, insisted on being transferred to another. Although the Committee say, the charges were not sustained, they admit this, and artfully omitting, in their reasonings upon it, any reference to the age and inexperience of the youthful incumbent, observe, that they do not believe, that because a man is born in New England he is disqualified from being a Professor in Cincinnati. If the Committee had been candid and intelligent on this matter, they would have admitted, first, that no young physician, with the ink not yet dried on his diploma, was qualified to teach the *practice* of medicine; and secondly, that the selection of a professor of the practice of physic from the state of Maine was injudicious, because no students could be expected from that quarter, and because its diseases differ in many respects from those of the West.

In regard to the second specification, it was proved, that the Trustees, as late as February 1831, had said to the Legislature, by a committee, that in no school, either East or West of the mountains, had the fees of tuition been put as low as in this. Here again the charge was sustained; and the Committee go on to justify the Trustees and Faculty. They cannot see in this underbidding, any consciousness of inferiority on the part of the Faculty; they cannot see, in the fact that the Transylvania School, refused, on this account, to acknowledge a course of lectures in the Medical College of Ohio, as equivalent to one in that school, any degradation; they cannot see that *poor* young men have, generally, had limited opportunities for preparatory education, and are likely to be imperfect in their scholarship and professional attainments, and, of course, not equal to the graduates of other schools, selected by the better educated; finally, they cannot see the testimony of the Editor on this point, as it was delivered, but condescending to play the demagogue, they make him say, that poor people do no credit to a school, and then put themselves forth as

the friends of the poor; when his opinion was, as just expressed, that poor scholars are, in general, of necessity, so superficially educated, that they cannot reflect upon the institution in which they graduate, the same credit they would confer if their early opportunities had been ample.

The fifth specification sets forth, in the beginning, that it was the *weakness* of the Faculty of the College for eight or nine years, that led to the projection, in Cincinnati, of a rival institution, under the auspices of the Miami University. Here, again, another charge was sustained, although the Committee declare, in the close of their report, that the "charges are not sustained." The evidence in support of this charge was ample. It consisted, in part, of letters, which were verified by their authors, and sent up by the Commissioners to the General Assembly, and of course were before the Senatorial Committee. The Editor will introduce some portions of them here. The testimony of Dr. Staughton and the Editor, who made a personal application to the Trustees of the Miami University, to establish a Medical Faculty, and of Dr. Eberle, who was one of its original members, may, perhaps, be considered conclusive on the point—that it was the weakness of the Medical College of Ohio, that invited the establishment of a rival school.

In his testimony, the Editor stated specifically, that but for the generally acknowledged insufficiency of the College, and the fact, that its Trustees had refused to re-organize it, he should not have attempted to project another. That Dr. Staughton thought in the same way, and predicated his actions of that conviction, will clearly appear from the following extract of a letter, written to a friend in the Eastern States, whom he solicited to unite in the enterprise. This letter was dated October 14, 1830, several months after Dr. Staughton's arrival in Cincinnati. It was verified by himself before the Commissioners, and the original is now in the archives of the Senate.

"Before this letter is placed in your hands you will have received from my friend, Dr. Drake, a communication on the subject of your removal to this city. The early and sincere affection which has united us, prompts me to present the subject to you in the plainest

manner possible. It is a *notorious fact* that the Medical College of this State, is in the hands of professors in *no way competent* to the discharge of the sacred trust reposed in them. This you know, and this I knew *before* I crossed the mountains; but I had *no idea* that it was *possible* for any men who held the name of Professors, to be so dead to *public opinion*, so *insensible* to those longings after professional *reputation*, which I once thought must necessarily be within the bosom of *every* public teacher. I would that I could find time and space to enumerate instances in proof, but it is enough that the *profession of the city feel* that the Medical School, in its *present condition*, is a *disgrace* to the State—it is enough that the community *feel* it. 'Sir,' said our most distinguished lawyer (Mr. B.) to me not long since, 'I feel *degraded*, as an inhabitant of Cincinnati, whenever I think of the manner in which our Medical School is conducted.'

"The question naturally arises, why then does not the Legislature look into the matter, and re-organize the School. Political motives, of a sectional character, have prevented, and most probably will prevent, till some public demonstration is made. Dr. Drake will explain this point more at large than I can do in a letter.

"Dr. Drake, with whose name and history you are perfectly familiar, has been looked up to by those medical men in the valley of the Mississippi, who have felt the *low estate of the Ohio Medical College* as the one on whom their hopes of a *better* state of things rested. From the intimacy which has subsisted between the Doctor and myself during my residence, I have no hesitation in stating, that I know of no one in the United States, so well qualified to undertake the foundation of a Medical School. To high and commanding talents, the Doctor adds a consummate knowledge of men. Naturally eloquent, he has cultivated his powers to an extraordinary degree. He possesses what both you and I prize not a little, amenity of manners, and kindness of heart. The great object has been before him a long time. With undaunted perseverance he has at last obtained the privilege of nominating the Professors of a medical school, a branch of the Miami University. I cannot enter into a history of this highly respectable institution. Its endowment of 22,000 acres of land will render it the *richest* seminary in the United States.

"Cincinnati is, beyond doubt, the place for a medical school. There *must*, in the nature of things, be a great college here. The present is the moment for the commencement. With the talents that it will be the Doctor's aim to enlist in the undertaking, there cannot be the slightest shadow of a doubt as to the *immediate* success. The proposition that will be made to you, must be sufficient to convince you of the confidence of the friends of the cause.

During the sojourn of the Editor in Philadelphia, when, as Dean of the projected Miami Faculty, he was seeking for colleagues, be received several letters from Dr. Staughton, designed to aid him in that public object. Most of them urged

on him the claims and qualifications of Dr. McDowell, then a private lecturer on Anatomy in Cincinnati.

In these letters Dr. S. expressed the results of his observations on the existing Faculty of the College. The following, from a letter now in the archives of the Senate, may serve as a specimen. It is dated November 14th, 1830.

"McDowell astonishes us:—His first lecture far, very far surpassed the College lectures; and on Friday I heard him give a lecture on the temporal bone, which was equal to any thing I ever heard on the subject.

"As to the College lecturers—Pierson was puerile to the verge of silliness. Smith's ideas were wholly from Abernethy, save and except a beautiful simile, between the functions of the system and the inhabitants of a city, which was emphatically *his own*. Slack lectured on galvanism, as a mode of curing all diseases, and on the divining rod!! Whitman I did not hear. The broad slang and low vulgar tales of Moorhead, reminded me of some of the discourses I had heard in the vagrant cellar of the Philadelphia Alms House. Cobb, for an introductory, read a translation of a description of a painted pasteboard man he bought during his seven-day sojourn in France, and after that the report of the committee of the House of Commons in England on the subject of dissection."

Dr. Staughton was afterwards a professor in the College, and that the distant reader may know whether his opinions, relative to the Faculty of 1830, are entitled to credit, the Editor will here reprint the following Resolution of the Board of Trustees, unanimously adopted on the occasion of the death of that Professor, August 9th, 1833, after he had been two years a member of the school.

"*Resolved*, That this Board learn with deep regret of the decease of Dr. James Staughton, Professor of Surgery in the Medical College of Ohio: a gentleman whose talents, acquirements, and moral and professional integrity, had secured to him the esteem of the public, and endeared him to the institution with whose prosperity he was so closely and warmly identified."

The following extract of a letter to the Editor, as Dean of the Miami Faculty, written by Dr. Eberle and dated Philadelphia, March the 4th, 1831, will disclose his convictions of the weakness and inefficiency of the Faculty of the College

as it then existed. It refers to an abortive effort of the Trustees of the College, to move the Legislature against the Miami school, in the preceding winter.

"The interest manifested in our behalf by your *medical* friends in Cincinnati and elsewhere, and the evident destitution of such support on the part of our enemies, is especially encouraging, as it betokens a kindly feeling towards our enterprize in the mass of the profession—the most certain evidence at once of the necessity and propriety of the effort we are making. The fact, too, of so large a portion of their own pupils signing our memorial, speaks loudly against the present school and in favor of our own undertaking. But the climax of damning circumstances, is the blasting report of the legislative committee—a formal acknowledgement and public declaration that the existing institution is wholly insufficient. Weak men should never provoke a contest; an ungenerous and unmanly opposition almost always defeats its own unhallowed purposes.

On the 31st of the same month, Dr. Eberle in another official letter, still speaks in a language that clearly indicates that his hopes of the success of the projected Miami School, were founded on the low estate of the existing institution.

"I have lately conversed with several gentlemen from Ohio—merchants in some of the interior towns; and they have uniformly spoken in the most encouraging terms of the enterprise we are engaged in. It does indeed, appear from what I have learned, that the present Faculty of the Cincinnati College, are by no means very formidable opponents."

In the succeeding winter, 1831–2, after several months acquaintance with the professors of the College, Dr. Eberle found his previous impressions in regard to one of them, Dr. Moorhead, the Professor of Theory and Practice, fully confirmed, for he declared to Dr. Martin, whose testimony went up to the Senate, that Dr. Moorhead was "30 years behind the profession."

It was in evidence, then, before the Senatorial committee, that those who projected the rival School in this city, were encouraged to do so, by the low reputation of the Medical College of Ohio in 1830; although it was then quite as high, perhaps higher, than it had ever been before.

The Trustees themselves, indeed, afforded a practical manifestation of concurrence in the same opinion, for before all the Professors of the new or rival School had reached Cincinnati, they displaced from the College one half of the incumbents, whom, in defiance of public opinion, they had cherished from 1823 or '24, a period of seven years.

Do not these different facts, then, clearly substantiate the heavy charge, that for many years the Trustees had kept in office, to the great injury of the institution, and of the interests of the profession, an incompetent Faculty? And yet the Senatorial Committee make the sweeping declaration, that the "charges against the State's agents were not sustained by the evidence!"

If no others were sustained, it is manifest that this was; and if no other had been, this alone would have kept a Committee, who respected the interests of the State, the rights of individuals, or their own character, from the sweeping verdict of approbation and eulogy which was reported to the Senate, *through* those to whom that body confided this matter.

Why! this was the very essence of the complaints of the medical profession, and of different societies in the State—that the Board had appointed incompetent men, who could exert no attraction on the pupils of the Valley of the Mississippi, who never investigated, who never wrote, who were unknown abroad, and *un*respected at home; and that it clung to them, until the establishment of a rival school, invited by their incompetency, compelled the Trustees to enter on a reform, or see the College utterly prostrated.

To this reform, the reader who may be inclined to pursue the subject, will now please to direct his attention.

On the 9th of April, 1831, Col. Davies, a member of the Board, and the brother-in-law of professor Pierson, presented his memorial, already mentioned, to the Senate, praying for a law to estop the Miami University in establishing a medical department. The Editor of this Journal, then returning from Philadelphia, happened at Columbus, and the next day presented a remonstrance. The committee to whom they were both referred, reported in favor of a new and enlarged board

of Trustees, and a re-organization of the faculty, which report was laid over till the first Monday of the ensuing December, and never called up. Before the Committee had reported, Col. Davies proposed that Dr. Eberle, Dr. McClellan, and the Editor, should leave their Miami colleagues and go into the College, with Drs. Pierson, Cobb and Moorhead. The Editor told him that he was under obligations, expressed and implied, not to abandon any of his colleagues, that he would respect the pledges he had given, and that, unless *Drs. Mitchell, Staughton and Henry were provided for, he would never consent to go into the College.* In the Spring the Trustees and Professors of the College made an unsuccessful effort to arrest the Miami organization, by a resort to the writ of *quo warranto.*

On the 21st of June, the Trustees declared all the Professorships vacant, and elected Dr. Mitchell to that of Chemistry, Dr. Staughton to Obstetrics, and the Editor to the Institutes, a chair created in 1827, but never filled. This left Drs. Eberle, McClellen, and Henry unprovided for; and as the Editor had declared, in Columbus, three months before, that his pledges to his colleagues put it *out of his power* to leave any of them behind, it was evident that he was offered the place, merely to amuse and delude his friends; with the certain knowledge that he *could* not accept it. As was his duty, he apprized the venerable President of Miami University of this offer, and received for answer, that "*he must on no account whatever desert any of his colleagues,*" but was told, as the opinion of the President, that if the whole of them were offered seats in the College, the Trustees of the University would be satisfied that they should accept.

The Trustees of the College, however, had no such intention. Their design was to detach Drs. Mitchell and Staughton. They believed, that although the Editor had refused to abandon those professors in the winter, *they* could be *induced* to abandon him; and the Trustees were not mistaken in the estimate they formed of the character of those persons, as we shall presently see. Finding, however, that Drs. Mitchell and Staughton were slow in coming over, and that they were anxious to have Dr. Eberle in the school, the Trustees

determined not to re-elect Dr. Smith, but to give his chair, Surgery, to Dr. Staughton, Obstetrics to Dr. Pierson, and Materia Medica to Dr. Eberle; Dr. Cobb was re-elected to Anatomy and Dr. Moorhead to the Theory and Practice. This left Drs. McClellan and Henry unprovided for; but the former was *willing* to remain in Philadelphia, and it was only necessary to secure a place for the latter. At this critical juncture, the Editor received a letter from Dr. Eberle, saying, that *Dr. Mitchell* had just written to him, that *he* and *Dr. Staughton* would enter the College, and leave Dr. Drake behind, if he, Dr. Eberle, would unite with them. This letter was verified and sent up to the Senate. Finding himself thus treacherously deserted, by those whom, but a few months before, he had peremptorily refused to abandon, the Editor had no resource, but to see the Miami Faculty, decomposed about him, or to suggest an extension of the Faculty, of the College from seven to eight professorships. This led him to propose the chair of Clinical Medicine, and to offer to take it himself, and yield the Institutes to Dr. Pierson, that Dr. Henry might have Obstetrics, the chair which he then held in the Miami School. The Editor would have preferred the Institutes to Clinical Medicine, but Col. Davies, the brother-in-law of Dr. Pierson, was opposed to the extension, Dr. Pierson had returned to his residence in New York, and the Editor felt convinced, that if he proposed such a limited and doubtful professorship, for the relative of a governing member of the Board, the whole plan of consolidation would fail; and, two of the members of the Miami Faculty leaving it, that project would be at an end. Therefore it was, that the Editor, proposed to take that chair himself, and that it should be left *optional* with the students to take or omit his ticket, expressing to his friends, that if he found the professorship too limited he would resign at the end of the first session. The Trustees were now concluded; but it was with great reluctance they confirmed the arrangement.

Dr. Pierson's appointment to the chair of Obstetrics, having been revoked in two days after it was made, was not sent to him. When the letter of the Secretary, informing him of his

transfer from Materia Medica to the Institutes, reached him, he declined accepting the latter, unless the Board would allow him a year for preparation.

The reception of this lettter was an important event in the progress of the school. The Board must either grant or refuse the request. They did not want eight professors. Dr. Pierson resided in New-York, and they were under no necessity of allowing him a year for preparation. If they did not grant him that favour, the union of the Institutes with Clinical Medicine, would form a good professorship, and the incumbent of the latter, who had been in some degree made a rallying point by those physicians of Ohio, who had labored to reform the College, would have been enabled to remain in it. But the Board did not wish that; and granted the request. The Session commenced, and the Institutes, *pro tempore*, were united with Clinical Medicine. From the limited extent of the wards of the Hospital, it was soon found, that Clinical Medicine could not be sustained as a separate chair. The students almost unanimously desired the two to remain united; but the Board clung to Dr. Pierson, and the Professor of Clinical Medicine, on whose application, twelve years before, both the College and Hospital, had been chartered by the Legislature, was compelled to resign. Such is the abridged history, of the consolidation of the two schools, as it was delivered in evidence before the Commissioners, and sent up to the Senate.

The resignation of the Professor of Clinical Medicine left seven professors, and called for nothing but the abolition of that chair. The Board, however, proceeded likewise to abolish the chair of the Institutes, then they displaced Dr. Henry, in whose favor nearly all the class had petitioned, and who was a native of the West; transferred Dr. Moorhead to his chair, gave the Theory and Practice to Dr. Eberle, and thereby opened the way for the restoration of Dr. Pierson to Materia Medica, after he had been eight months preparing himself for the Institutes! Thus two Western Professors were sacrificed, and one important chair abolished, to bring back to Materia Medica, a physician resident in the city of New-York, because he was the brother-in-law of an influential

member of the Board, who was on *every* Committee which had these complicated and sinister movements in charge. The evidences of all of this, again, were before the Senatorial Committee, when they declare in opposition to the facts, that the "charges against the states' agents were not sustained."

But the Senatorial Committee in their partizan zeal, have gone still farther. They have asserted that Dr. Pierson received his appointment, with the express stipulation, that he should be absent; which means, that he should stay away to prepare himself;—but no such stipulation was made or sent to him with his appointment, and no such evidence was before them. Honorable men, whether in a private or public capacity, ought to feel desirous to explain away or correct such an error.

It was these various manœuvres, that stirred up anew, the members of the Third District Society, and the General Medical Society of the state, and led to the memorials on which an inquiry into the conduct, management and actual condition of the Institution; but after it was made at an expense of many hundred dollars of public money, the members of the General Assembly, never read the testimony, and voted that all had been properly done and that all was well. How far the facts that have been detailed, go to show partiality and favoritism, in the Board, every reader must judge for himself.

Immediately after the resignation of the Professor of Clinical Medicine, Dr. Pierson's brother-in-law, wrote and published in one of the papers of the city, a piece designed to show, that the College which then existed, was not the same corporation which had been founded by that Professor; and in two or three months, an advertisement, signed with the names of the President and Secretary of the Board, appeared in the gazettes, asserting the same thing. All the facts, however, demonstrated the reverse, and the Commissioners declared against this unjust and ridiculous pretension. It was set up to reconcile the friends of the Professor of Clinical Medicine, to the course of policy which had compelled him to resign.

The advertisement just mentioned, deserves a further no-

tice. It praised the Faculty and called on the community to extend its confidence to them; it was in short a kind of eulogium and certificate of character; and might have been of service to them in after times, if it had not, unfortunately, come out in evidence before the Commissioners, that neither the Board of Trustees, nor the President and Secretary, whose names were appended to the paper, had ever seen it! The Faculty had got it up themselves, and sent it forth to the world under the signatures of persons who had not even heard of it, till they read it in the newspapers! This is one of the examples cited in the fourth charge. Another was the annual report of December, 1832, to the General Assembly, on the state of the Institution, which, as appeared in evidence, was never seen by the Board, and, in fact, but by one of the Committee of two, whose names were attached to it! Another instance of a similar kind, referred to in the same (fourth) charge, was a printed address to the Legislature in the following February, 1833, containing a great variety of misstaments, avouched by the names of the whole eleven members of the Board, including one who was then a Senator in Columbus; although it was proved before the Commissioners, that it had never been read in the Board, and was not even drawn up by a Committee appointed by that body, but by some individual member, and the names of the Trustees affixed to it, without a part of them at least, ever having heard it read! All this was known to the Senatorial Committee, and still, they have the pliancy to tell the Senate, that the "Charges against the state's agents are not sustained by the evidence."

The restrictions which the limits of this Journal impose, require that this article should not be extended much farther, and many other charges, therefore, can only be referred to.

The 9th, 13th and 15th specifications under the first charge, were fully sustained; and in regard to the 9th, it was shown, that the Faculty had made a deceptious return to the Legislature of the number of pupils for 1832–3.—Still, the "Charges against the state's agents were not sustained by the evidence."

Both the specifications under the second charge were fully sustained. The third charge has been already mentioned.

The first and second specifications under the fourth charge, have been likewise commented on. The third, fourth, fifth, sixth and seventh, specifications under the same charge, were amply supported; and the eighth, ninth and tenth, were rendered highly probable.

The first of the charges against the "Faculty of the College," p. 629, was established in the most unquestionable manner. The second, third and fourth were supported by a variety of of testimony. The fifth and eighth were proved.

The whole of the "Additional Charges against the Trustees and Faculty," p. 630, were supported by testimony the most conclusive. The two first have been already referred to. The third and fourth were made out by irrefutable proofs.

For the truth of all that is here asserted, the Editor solemnly appeals to the testimony taken down by the Stenographer of the Commissioners, and deposited in the archives of the Senate—unread by that honorable body, and placed beyond the reach of the people. It may seem strange to the readers of this Journal, who are physicians, that the Senate could thus lose sight of its dignity and duty; but a fact or two will dissipate the mystery.

1. An old and influential Senator, himself both Physician and Lawyer, is a Trustee, and could of course tell the Senate how to act; if, indeed they could have been so deficient in courtesy, as to do or propose any thing that might affect his feelings as a *nominal* member of the Board. 2. The country members of the General Assembly leave the affairs of the College, to the decision of those from the city where it is located; and it does not require Major Downing's slate and pencil, for them to calculate the difference, *on election days*, between the votes of eleven Trustees and six Professors, with their friends and dependents, and *one* man,—unsustained by any faction, unallied to any cabal, and occupied in writing dull editorials for the Western Journal.

Towards the close of their report, the Senatorial committee observe that,—

"That they have no doubt of Dr. Drake's qualifications and believe he is capable of doing honor to any Medical School. But they

are compelled to say *from the facts disclosed in this case*, that it is extremely problematical, whether peace and harmony could prevail for any length of time, in a Medical Institution with which he should be connected, unless it was entirely under his controul and management."

How must the different Faculties to which Dr. Drake has belonged, congratulate themselves that they should have got rid of him, before their "peace and harmony" were broken up! That he destroyed the "peace and harmony" of none, was in evidence before the Senatorial Committee. That he had been *twice* a member of Transylvania University and lived in "peace and harmony" with the Faculty and Trustees, was among the "facts disclosed in this case:"—That he had lived in "peace and harmony" with the Faculty of Jefferson College, in Philadelphia, and after his resignation, was earnestly invited to return and assist in a reorganization of the School, was among the "facts disclosed:"—That Dr. Eberle, after he had left the Jefferson School, in writing to a friend, spoke of Dr. D. as "a most worthy and honorable individual, who in the whole course of his life had maintained the character of a high minded, honorable, upright and intelligent man," with whom he was anxious to be again associated, the projected Miami School, was among the "facts disclosed:" —That during the brief existence of that Faculty, he lived in "peace and harmony" with its members, was among the "facts disclosed:"—That during the whole time he belonged to the College, after the consolidation of the two Faculties, he lived in "peace and harmony" with them, was among the "facts disclosed." If the Senatorial Committee chose to express the opinion which they did, without referring to the "facts disclosed," he certainly would make no objection; but when they pretend to base that opinion upon the "facts disclosed," he is at liberty to say, that by a slip of the pen, they have written the opposite of the conclusion which the "facts disclosed" require. No doubt the members of the Committee are all honorable men in private life, and would not willingly pervert or suppress the truth, for the purpose of calumniating a fellow citizen; but who is so simple minded as to suppose, that legislators and politicians are expected to ad-

here to the "facts disclosed?"—If this were done, the "peace and harmony" of factions would soon be annihilated.

POST SCRIPT.

As a specimen of the gossip with which the Trustees and Faculty amuse the General Assembly, in their annual Reports on the progress, state and prospects of the College, the Editor gives the following extracts from those of December 1832 and 1833.

REPORT OF 1832. "*Carthago est delenda.* The Great Apollo has issued his behests from his tripod in this city, and his little but faithful court of sub deities are raising the shout and pointing their shafts at the devoted heads of the Trustees and Faculty of the Medical College of Ohio"!

It was certainly most humane in the Senatorial Committee, to cast the ægis of the State before the devoted heads of these worthy gentlemen whom Dr. D. is always persecuting.

REPORT OF 1833. "But the cause which has operated with the greatest effect in producing this result, is, the continued efforts of the enemies of the Institution, to injure its reputation abroad, and to persuade students who had determined to enter this College to change their determination, and repair to other Institutions. It is a fact, of which there is ample proof, that several students who had arrived in this city for the purpose of joining our Institution, was told, that it was in a sinking condition, and that they would, if they consulted their own interests, leave the city and enter the College at Lexington. This advice, we know, had its intended effect in several instances. We also know, that letters have been written by medical men in this city, to others in Kentucky, misrepresenting this Institution, and advising them to send their students to other schools. Thus it appears, that these persons by propagating slander, prevent the collection of a large class, and afterwards resort to *that fact*, in proof of the slander. Had it not been for these, and similar measures, there is reason to believe that the class, at this time would have amounted to two hundred."

A LETTER FROM PROFESSOR MITCHELL.

To the Board of Trustees of the Medical College of Ohio. Gentlemen:—It is proper that I should communicate to you the following information. Sometime in the first or second week in November, of the present year, Mr. William J. Barbee, a student of Medicine from Paris, Kentucky, stated to me, that his preceptor Dr. Telford, a few days or weeks before he, (the said Barbee,) came to Cincinnati to attend the lectures, had received a letter from Dr. Drake, which represented the prospects of a class in the Medical College of Ohio, for the approaching session, as very doubtful, and *advised Dr. Telford to send his pupils to the Lexington school.* I have further to state, that two students from Washington, Kentucky, who had come here for the purpose of

deciding whether they would become pupils of this school, or go to Lexington, stated to me, as one objection to remaining here, that it was currently reported in their town, that a course of lectures in this Institution would not be credited in another school. It was in the presence of these students, that Mr. Barbee made the statement above referred to. The information which the Washington students obtained, touching this difficulty, satisfied their minds, and they immediately joined the class. It is proper here to state, that the report put in circulation at Washington, has no foundation in truth, although its tendency to prevent students from coming to the Medical College of Ohio, must be obvious to every one. From my conversation with the Washington students, I was satisfied that they would have gone to Lexington, if they had not resolved to take Cincinnati in their route for the purpose of obtaining information.

Very respectfully,

THOMAS D. MITCHELL, Dean, &c.

Cincinnati, Dec. 4th 1833.

FROM PROFESSOR MOORHEAD,

To the Secretary of the Board of Trustees of the Medical College of Ohio.

Cincinnati, Dec. 4th 1833.

Sir, Some time about the middle of October last, three students from the South, came to this city, for the *purpose* of entering the Medical College of Ohio. They took their temporary residence at the Tavern of Mr. Heckewelder, while there, they were visited several times by Dr. Drake, to whom, I understood, they had letters of introduction. After these visits, they left the city and went to the school at Lexington, with letters of introduction from Dr. Drake. For the truth of this statement, I refer you to Mr. Heckewelder himself, from whom I received the information.

To the Secretary of the Board of Trustees.

I am, Sir, very respectfully your obedient servant.

JOHN MOORHEAD.

FROM PROFESSOR COBB.

To the Trustees of the Medical College of Ohio.

Cincinnati, Dec. 4th, 1833.

Gentlemen.—I was lately informed by Mr. Nathan Guilford, that a Mr. Ford, of, I think, Georgetown, Kentucky, on his way to this city, met at the tavern where he spent the night, three or four Medical students, who were going to Lexington to attend the lectures. They informed him, that they had just visited Cincinnati for the purpose of attending the lectures in the Medical College of Ohio, but that Dr. Drake advised them to go to Lexington, alleging that the Medical College of Ohio, was in a sinking condition, and that there would not be 25 pupils to attend the lectures the ensuing session.

The Honorable the Board of Trustees of Medical College Ohio.

JEDEDIAH COBB.

Now when the Commissioners of inquiry were in session, Dr. Martin sent to them an affidavit, which contained some things communicated to him by Mr. Ellsberry; but the Committee of the Board of Trustees expressly objected to them because they were *hearsay*, declaring that the Trustees could not consent to be condemned on hearsay testimony. It seems, however, that the honorable Board think that kind of testimony good enough for Dr. Drake, even when the witness, unlike Dr. Martin, who quoted a member of the Senate, is unable to give the name of his informant. It is edifying to see respectable men involve themselves in such inconsistences.

But these certificates demand and shall receive a different kind of notice.

The letter of the Editor, to Dr. Telford, a former pupil, of Troy in this State, not Kentucky, was written October 22d, only six days before the lectures in the Medical College of Ohio commenced, and was in reply to one from that gentleman, dated September 18th. Not having kept a copy of it, the Editor has sent to Dr. Telford on the occasion, and will here present the following extract from his reply:

"The case of Wm. J. Barbee was simply this: Some time last summer, his mother wrote me, asking my advice where, in the West, he had better attend a course of lectures, and desiring me to obtain your opinion. This, and nothing else, gave rise to the correspondence. I frankly state to you, that all the circumstances of the case considered, I advised Mr. B. to go to the Lexington school. I then thought, and now think it the best school, and I so stated to you *my opinion*, not dreaming it was to be made a public matter, and the ground of a charge against you. At your request, I here copy (word for word) all in your letter of the 22d October, relating to the Medical College of Ohio.

'It is generally believed here, that the Commissioners of enquiry into the state of the Medical College of Ohio, will report in favor of enlarging the Board. Should they do so, and the Physicians of the State think proper to exert themselves with their Representatives, important results may follow.

'As yet, I believe very few pupils have arrived here, and two who had been clamorous friends of the reigning dynasty have lately set off for Philadelphia.' This is every word of the correspondence."

The letter in answer to Dr. Telford's, was the only one which the Editor wrote to any person on that subject, during

the vacation of that year. Further, Mr. Barbee denies the most important part of Professor Mitchell's certificate:—

Cincinnati, Ohio, March 21st, 1834.

DR. DRAKE,

SIR,

I wish to say that I never stated to Dr. Mitchell or any other individual, as he (Dr. M.) has represented in his letter of December 4th, 1833, to the Board of Trustees of the Ohio Medical College, and published by them in their annual report, that you had advised my preceptor, Dr. Jno. G. Telford, by letter or otherwise, to send his pupils to the Lexington School in preference to that of this place; nor have I ever understood or stated that you gave such advice to any individual whatever.

Yours respectfully,

WM. J. BARBEE.

The Professor is not *quite* perfect, but he should not be blamed over much, for the sentiment of the College is, that in every thing which respects Dr. Drake, the end will justify the means. The Professor is not altogether a novice at *such* mistakes. In the summer of 1831, when the consolidation of the Miami Faculty with that of the College was a matter of negotiation, the Professor wrote to Dr. Eberle, then in Philadelphia, that the "Trustees of the College wished him (Dr. E.) to take the chair of Obstetrics," but within thirty days forgot it, and declared that he had never written any thing of the kind, nor even mentioned that chair, and that Dr. E. must be out of his senses!

The Professor attempts by implication, to show that Dr. Drake had spread a report, that the Lexington School would not recognize a course of Lectures in the Cincinnati. This was the case when Dr. D. came into the College in 1831. It was then rescinded. For the fact, that some students had not yet come to a knowledge of the repeal of that regulation of the Kentucky School, Dr. D. is to be held responsible to the General Assembly!

The Professor is really a *good* man, but a man cannot be *good* always. *Maculæ soli sunt*—the sun himself hath spots. But for one little spot, the Professor's moral character would be still better than it is.

Professor Moorhead's certificate, *happens* to contain one

small truth, which is, that Dr. D. gave the Mississippi students letters of introduction; but he did not advise them to go to Lexington, he did not know where they lodged, and did not call on them. In their first visit to him, they informed him that they were on their way to other schools. When they first landed, and before they had seen Dr. D. they told Dr. Dodge and Dr. Wood the same thing. What language they may have held when questioned by grooms and barkeepers on behalf of the College, he does not know.

Professor Cobb's certificate is a choice mess of stuff for a *gentleman* to put his name to. Several students, whose names are not given, in a tavern not designated, told a Mr. Ford, who told Mr. Guilford, who told Dr. Cobb, who told the Trustees, who told the General Assembly, who told the Public,—that Dr. Drake had advised them to go to Lexington, for that the Cincinnati School was in a sinking condition and would not have 25 pupils! The whole is utterly false, whether these unknown students, a Mr. Ford, Mr. Guilford or the Professor was the original author!

It is but an act of justice to the President of the Board, to state that he refused to sign the annual report to which these various puerilities were attached.

Dr. Drake, in conclusion, wishes to say, that after he was called as a witness before the Commissioners, in April last, and delivered his testimony, he left the whole matter to the Legislature, and neither by written or spoken words, advised any physician or student, in favor of the Lexington or any other school, in preference to that in Cincinnati. On the contrary, six of his pupils were connected with the latter, a greater number than any of its professors furnished. Notwithstanding all this, however, it seems that Professor Mitchell has received intellegence that Dr. Drake's exertions kept away 88 pupils. This is truly paying a splendid compliment to his standing in the Valley of the Mississippi. While engaged in the drudgery of professional business, and remaining constantly in one town, to direct the choice of 88 students, scattered over ten or twelve states, is a moral influence which few men in the profession have ever wielded! No wonder the Faculty

and Trustees, have invoked the aid of the General Assembly against this talismanic power!

With these remarks the College is dismissed till the next number.

II. Popular Study of Anatomy and Physiology.

The Editor after having been repeatedly urged by a number of respectable teachers, of both sexes, has commenced the composition of a school and college book of *Anatomy, Physiology and Hygeine.* It will run through about 300 duodecimo pages, and be illustrated with numerous engravings on wood.

The organs and functions which are not of general interest, and those which cannot, with propriety, be introduced into a work designed for all kinds of readers, will be omitted, together with all surperfluous technical terms.

Having stated what the work will *not*, it may be well to say something of what it *will* contain.

An account of the skeleton, with observations on its mechanism, and deformities—muscular motion, with the influence of exercise—the brain, with an accout of the science of phrenology—the distribution and classification of the nerves—connexion between the nerves and the muscles—the senses—the intellectual functions, with an account of the relation of mind and body on each other, and the connexion of physiology with mental physiology—the heart and circulation of the blood—action of the blood upon the brain—the lungs and respiration—connexion between the functions of circulation and respiration—the stomach and liver—digestion—bilious secretion—the secretions generally—influence of the digestive functions over those of the brain and *vice versa*—effects of intemperance in eating and drinking—diseases which are the offspring of civilization—pernicious effects of premature or excessive study—of excited passions—of sedentary habits and occupations and of confined air, &c. &c.

The Editor wishes, in this notice, to make an appeal to the profession in favor of his undertaking. Believing that Anatomy and Physiology, to a certain extent, should be made a

study in every literary institution which teaches Chemistry and Natural Philosophy, but aware, that not a little of popular prejudice must be encountered, he would, respectfully, solicit the aid of his professional brethren. They may do much to promote the introduction of this study into our higher schools and colleges; and they are deeply interested in making it a branch of general education. It is now a universal complaint, in the profession, that the people do not distinguish between its learned and its ignorant members. This they cannot do, from the want of knowledge themselves, but supply that want, make them acquainted with the first principles of Anatomy and Physiology, and they will soon come to discriminate between the man of science and the empyric. But to take a less selfish view of this matter, it may be said, that if it would be beneficial to society, to have Anatomy and Physiology made a branch of liberal education, physicians should interest themselves in the matter, because their efforts would have greater effect, than those of any other class of men.

In the next number of the Journal, the Editor proposes to give a specimen of the work, that his brethren may judge for themselves, of its adaptation to the end in view.

III. Epidemic Cholera.

It appears by the gazettes, that an occasional case of Cholera occurs on board the steam boats, which ply between New-Orleans and Cincinnati.

In this city, about the time of the equinox, when a thunder shower was succeeded by great depression of temperature, several cases of diarrhœa, and one or two of fatal Cholera occurred. A week has since elapsed, without any subsequent death, and indeed, there does not at present seem to be any tendency to the Epidemic.

IV. National Marine Hospitals on the Mississippi and its Tributary Streams.

Dr. Cornelius Campbell, a respectable physician and public spirited citizen of St. Louis, has been for some time enga-

ged in arousing the attention of his friends and acquaintances in the West, to the great importance of such establishments. Our limits in this number, do not permit our presenting to the profession in the Valley of the Mississippi, even an outline of his views; but we propose to make them the subject of an article in the next number. Meanwhile we hope that every reader, will give to the matter that consideration which its importance obviously demands, and be prepared to lend his aid, in an application to the General Government, whenever the time for making it may arrive.

V. Inquiries Concerning Tubercular Diseases.

We have received from Dr. James Jackson, jr. of Boston, a printed circular of Dr. Clark, of London, containing the following inquiries relative to tubercular diseases, and especially tubercular consumption. Dr. C. is a physician of learning and eminence, already favorably known as a writer on the Influence of Climate, and the subject of his present inquiry is one of deep interest. We take great pleasure in presenting his queries to our reader, and beg leave to add the expression of a hope, that several of them will be found qualified and willing to aid in the inquiry. Their communications may either be sent (post paid) to the Editor of this Journal, or by private hand to the post office in New-York, directed—"James Clark, M.D. Author of a Treatise on the Influence of Climate, London, Great Britain." Dr. Clark is desirous of receiving early replies.

1. Do you consider Tuberculous Consumption, and Tuberculous or Scrofulous diseases generally, more or less prevalent among the *opulent classes** of society at the present time than† years ago?

2. Every medical man of observation must be more or less acquainted with the state of health of three generations:—What is the comparative state of health of these respectively?—Is the general standard of health higher or lower—and are Tuberculous‡ diseases more or less prevalent in the rising generation, than in the two preceding generations?

*By the opulent class are meant all those enjoying the comforts of life.

†The space to be filled up according to the extent of your experience.

‡The terms Tuberculous and Scrofulous are used synonymously.

3. To what circumstances do you attribute any change which you may have remarked in the state of health of the more opulent classes?

4. What do you consider the relative prevalence of Scrofulous diseases among the *lower orders** of society at the present moment, and† years ago.

5. Do your observations refer more especially to any particular class of people?

6. To what circumstances in the condition or habits of the people do you attribute any change in the state of their health, which you may have observed?

7. Have any particular occupations appeared to you to give rise to Tuberculous Consumption, or to Scrofula, in a marked degree?—If so, specify them.

8. Have you remarked any occupations which give an evident, comparative, exemption from Consumption or Scrofula?—If so, specify them.

9. Have you observed Tuberculous diseases to prevail more or less in particular localities?—If so, specify them.

10. Have you observed Tuberculous diseases to prevail very generally among spirit-drinkers?

11. What do you consider the chief causes of Scrofulus diseases generally, and of Consumption more particularly, among the working classes?

12. What is the prevailing form which Scrofulous disease assumes, and what organs or structures does it chiefly affect, in the district under your observation?

13. In the course of your experience, have you observed any change in the prevailing type or forms of Tuberculous diseases? Are the internal or external organs, comparatively, more or less frequently affected now than they were formerly?

14. Have you observed any difference of character between the Tuberculous diseases of the opulent and the working classes generally? or between the diseases of *the children* of these two classes more particularly?

15. Have you remarked a connection between any particular derangement of health of the parents, and Scrofula in the offspring.—If so, specify it.

16. Have you observed any derangement in the general health, or any disorder of particular organs of individuals to precede the appearance of Tuberculous diseases in them?—If so, specify such general or local affection.

17. At what period of life have you observed Tuberculous Consumption most frequently to make its appearance?

18. Have you observed any particular diseases to affect persons of a Scrofulous habit, after they have reached their 50th year?

***Under this title, include all those who gain their livelihood by bodily labor chiefly.**

†The space to be filled up according to the extent of your experience.

Art. VI. Obituary Notice.

*Dr. Sanders.**—Among the numerous victims who have fallen beneath the pestilence that lately passed through our country, spreading wo and desolation in its progress, perhaps none is more deeply lamented than *Thomas Sanders, M. D.*, who departed this life at his residence in Shelbyville, Ky. on the 21st of July, 1833, aged 39 years. It is a sad and painful task to record the untimely death of an early and affectionate friend, but the ties which united us whilst living, and a custom, founded upon the benevolent and sacred feelings of the human heart, call for this humble tribute to his memory. When the silent tomb closes over objects of the most tender regard and cherished affection, there is no consolation left us, but to dwell with melancholy pleasure upon their memories.

The subject of these remarks, received the honours of Transylvania University, in the year 1824. Inquisitive, ardent, and aspiring, he located himself in Shelbyville, and commenced his professional career. Endowed with intellect of a superior order, and stimulated by a spirit of the most laudable ambtion, he embraced his profession with pride, and pursued it with the eagerness of a true lover of knowledge. The narrow and circumscribed paths of the science could not confine him. He aimed at the highest walks of the profession. He regarded the science of medicine as a science of the noblest order, fraught with the destiny of millions, and capable of dispensing blessings or heaping curses upon the human race. With this enlarged view of the profession, he strove to ameliorate the condition of all about him. The subtleties that lie concealed in the labyrinth of medical lore, the revelations of nature and experience, and the developments of science, were suoght after and mastered, with the eager and untiring spirit of a philanthropist. But in the prime and vigor of manhood, upon the threshold of professional usefulness, and just beginning to reap the harvest of his early devotions, he was suddenly arrested in his earthly pilgrimage, and cut off from accomplishing the task he had so nobly begun.

* From a Correspondent.

In the death of Dr. Sanders the society where he lived is bereaved of a highly valued and much esteemed member, and the Faculty has lost one of its brightest ornaments.

VII. Meteorological Register.

With the subjoined abstract, we commence the publication of a register of the state of the Thermometer, Barometer, Winds, Clouds, and Rain, kept at the Woodward High School, in the northern part of this city, lat. 39°.6′ and long. W. from Greenwich 84°.22′. The observations are made by Joseph Ray, M. D. one of the professors of that flourishing Institution; who to his professional attainments, adds a knowledge of Mathematics, Astronomy, and Physical Geography, and is, therefore, likely to make observations on which reliance may be placed.

Abstract of Observations.

Thermometer.	Jan.	Feb.	March.
Mean temperature	28°.78	45°.29	43°.76
Maximum	62°.	66°.	70°.
Minimum	0.	22°.	20°.
Range	62°.	44°.	50°.
Barometer.			
Mean height	29.473	29.278	29 339
Maximum	29.800	29.600	29.770
Minimum	28.260	28.630	28.920

Remarks.—The month of January was 11 degrees colder than the same month of the preceding year. The month of February was remarkable for regular temperature and pleasant weather; the number of clear days being 16 and that of the cloudy 12. In March the number of clear days was only 11. January, this month the direction of the wind was West 21 days, South 3 days and the remaining part of the month North and North-East. The whole depth of rain in this month was 3.87 inches. During the first two months of the year, no regular account was kept of the direction of the wind or the quantity of rain, this will be done in future. JOSEPH RAY, M. D.

VIII. Specimens of Plants.

The Curator of the Cincinnati Medical Society's Herbarium, in behalf of that Society, returns his grateful acknowledgments to those who have contributed dried specimens of plants, for the commencement of an Herbarium. Its enlargement still depends on the voluntary contributions of individuals; and all who have it in their power to aid in the object, will do the Society a favor, to forward to the Herbarium, such plants as they have in their possession, or can collect.

It would be desirable, so far as may be, that all plants should be collected while in flower; and those which have a succession of flowers, when the first have passed into the early stage of fruit, that the germ may be seen. The herbaceous plants, when not too large, should be taken entire with the root, that the radical leaves may be exhibited; and those which are large, should be accompanied with radical leaves, taken from a plant of younger growth. Many plants cannot have their characters fully exhibited without two or more specimens taken at different stages of frutification. In collecting a plant, it should be the object to exhibit all its diversity of leaves, its flowers both staminate and pistillate, and when it can be, its fruit.

Plants should be dried by absorption between papers, so as to show as near as can be, their natural form and original color. A large quantity of paper, the more the better, should be interposed between them, and a weight of from fifty to eighty pounds applied, and the papers should be changed and well aired every day, till the plants are thoroughly dried. Common wrapping paper does very well.

It would also be desirable that every plant presented to the Herbarium, should be accompanied with a label, containing its locality, time of flowering, habits of growth, the common names, its medical virtues, &c. Plants will, however be very acceptable without any label. Each plant presented, when placed in the Herbarium, will be labelled with its name, the class and order, time of flowering, locality, its medical properties if any, and the name of the donor.

If any persons should have plants whose names they are un-

able fully to make out, and should be disposed to send their duplicate specimens, numbered, to the Herbarium, accompanied with such remarks respecting the habits of the plants as may occur, the Curatior will take great pleasure in examining them and will, if in his power, return the names attached to the numbers, to those who sent them.

The gentleman in Wheeling, Va. is informed that his unnamed plant is of the genus Euphorbia; but as it was but the part of a plant, and it did not appear certain whether it was annual or perennial, erect or procumbent, the species was not so clearly made out, it is however most probably pilosa—*Euphorbia pilosa.*

For the examination of plants, Nuttall's genera, Torrey's compendium, and Eaton's manual are perhaps the best small works. Nuttall contains nearly all the genera that will be found in Ohio and adjoining states. Torrey, though somewhat deficient in the number of the species, is nevertheless plain and familiar in his descriptions. Eaton contains the fullest catalogue of plants, but has so far simplified his descriptions, that some of the characters necessary to distinguish one species from another, are left out. In addition to these, some elementary work will be necessary for the definition of terms.

ISAAC COLBY, M. D. *Curator.*

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